Three Essays on Child Maltreatment Prevention

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ABSTRACT

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This dissertation includes three papers that examine the role of antipoverty policies and programs in preventing child maltreatment. Paper one examines how access to Medicaid impacts child maltreatment as characterized by Child Protective Services (CPS) reports. Paper two considers how access to Early Childhood Education and Care (ECEC) programs affects child welfare involvement. Paper three assesses the relationship between temperature and CPS reporting, looking to air conditioning and state LIHEAP cooling policies as a potential source of mitigation.

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III. ACKNOWLEDGEMENTS

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IV. DEDICATION

To Greg, Geoffrey, and James, my life, my sweetness, and my hope.

V. PREFACE

This dissertation includes three papers that provide quasi-experimental evidence on new policy solutions for maltreatment prevention.

Impoverishment is consistently associated with Child Protective Services (CPS) involvement, often experienced in conjunction with other forms of hardship that degrade the environmental and internal stability of the family, such as diminished mental and physical health and decreased labor force participation. By extension, ‘Parenting in despair’ – that under high levels of stress and depression coupled with constrained resources – may be amenable to policies and programs that ameliorate the parenting environment. Indeed, numerous causal studies have shown that the generosity of programs such as AFDC/TANF, EITC, and minimum wage appear to drive down CPS report rates. Yet, the maltreatment benefits of three of the largest social antipoverty programs remain unknown. The aim of this dissertation is to estimate the prevention effects of these policies.

My first paper, “The Effect of Medicaid on Child Maltreatment: Evidence from Early Expansions in California” provides the first evidence of a reduction in maltreatment due to an exogenous increase in access to Medicaid through California’s early expansions using administrative data. I find that access to Medicaid reduced child physical abuse reports by up to 11 percent. I detect much larger effects among children from historically disadvantaged homes, suggesting that Medicaid indeed improves child safety. My findings imply that Medicaid may indeed serve as a policy vehicle to improve family functioning and child safety.

My second paper examines the role of Early Childhood Education and Care (ECEC) programs and child maltreatment. In this paper, entitled, “ECEC Programs in the United States: Does

Access Improve Child Safety?" I exploit the temporal and spatial variation in enrollment and subsidy shocks of the three largest, publicly funded ECEC programs to estimate their benefits in terms of four proximal measures of child maltreatment. My results imply that programs for young children (infants to two years old) may offer the largest welfare gains, though I find weak overall evidence of a benefit to expanded program access. That I fail to detect a lower-bound effect of ECEC programs in general gives further evidence to the notion that program quality appears paramount to expanding access.

The findings from these papers imply that access to Medicaid affects child maltreatment through a reduction in stress and depression and through a contemporaneous increase in income, though I am unable to parse out explanatory mechanisms. For my third paper, I examine temperature as an interesting test for the stress pathway, as it is both exogenous and does not affect income (in the short run). To that end, "Hot Tempered: New Evidence on Temperature and Child Maltreatment" uses exogenous variation in temperature and acute heat exposure to provide the first estimates of the effect of temperature on child maltreatment among young children. I examine whether metropolitan-level air conditioning penetration rates and state-level LIHEAP cooling programs plausibly mitigate this relationship. I find that a 10 degree increase in temperature is associated with at minimum, a 4.7 percent increase in reports. While a/c penetration rates reduce the effects of temperature by up to 50 percent, existing LIHEAP state cooling programs fail to have an effect. These findings suggest that LIHEAP, or other programs that promote access to cooling, should be expanded to reach more low-income families with young children.

Overall, my findings suggest that improving the context of parenting is not ignorable among myriad policy solutions. That policies such as Medicaid, ECEC programs, and LIHEAP can improve child safety merits consideration among the possible approaches to prevention. Further, that a lack of access to the benefits remitted by these programs can increase maltreatment yields invaluable information for practitioners, underscoring the importance of such factors to child safety.

VI. *Paper 1: The Effect of Medicaid on Child Maltreatment: Evidence from Early Expansions in California*

Abstract

In this paper, I examine the effect of access to Medicaid on child maltreatment rates using administrative data capturing the full census of alleged child maltreatment reports in the U.S. between 2010 and 2013 (N= 4,755,579). To identify the effect of Medicaid, I exploit the exogenous variation in access to Medicaid by the county-level early expansions in California's Low Income Health Program (LIHP) from 2011 – 2012. My most conservative estimates suggest that access to Medicaid significantly reduced reports of physical abuse by up to 11 percent. I detect ample effect heterogeneity, with larger effects among children from families with financial hardships and those historically disadvantaged. This paper provides new evidence to inform the Medicaid discussion, providing new evidence suggestive of the potentially costly consequences of a retraction of benefits or generosity.

Introduction

Child maltreatment in the United States is increasingly prevalent. An estimated 37.4 percent of children experience an investigation by Child Protective Services (CPS) before their 18th birthday (Kim, Wildeman, Jonson-Reid, & Drake, 2017). In 2016, states received 4.1 million CPS referrals for 7.4 million children, a 10 percent increase since 2012 (U.S. Department of Health & Human Services Families, Administration on Children Youth and Families, & Bureau, 2016). As many adulthood inequalities appear to manifest in childhood, preventing early maltreatment is paramount (Almond & Currie, 2011; Almond, Currie, & Duque, 2017; Currie & Rossin-Slater, 2015; Heckman & Masterov, 2007). Children under 12 months in particular face the greatest risk of undetected maltreatment, the consequences of which are grim. Not only do the youngest children have the highest victimization rates (24.8 per 1,000 children), they account for half of child maltreatment fatalities (U.S. Department of Health & Human Services Families et al., 2016). If the effects of maltreatment nearly disappear if treated before the age of two, as suggested by a long-running study of orphans in Bucharest, the long-run benefits of early intervention are likely vastly underestimated (Nelson et al., 2007).

One fundamental challenge is to provide early and generous access to preventative services. An estimated 1.9 million children received prevention services in 2016 with another 1.3 million receiving post-investigation services,[1] implying that the majority of victims – and the much larger population of unsubstantiated victims and those not captured by official statistics – never receive stabilizing support. The Family First Prevention Services (FFPS) Act of 2018 aims to broaden access to preventative services, allocating funding to States for the title IV-E reimbursement of substance abuse and other mental health services along with parental training. With the exception of home visiting programs, however, there is limited evidence as to the efficacy of most preventative programs (Levey et al., 2017). Further, as these and other preventative services are often prioritized by observed risk, meaning that receipt is contingent upon a prior interaction with CPS, researchers have turned to policy-based universal prevention strategies. Despite the fact that child maltreatment is epidemic in proportion with an estimated cost of up to $30 billion annually – with an additional $124 to $585 billion lifetime cost of each

[1] https://www.childwelfare.gov/pubPDFs/canstats.pdf

new case (Fang, Brown, Florence, & Mercy, 2012) – relatively little is established in the causal sense about large-scale policy solutions outside of those targeting parenting behavior.

As poverty is the most prevalent risk factor for child maltreatment, a key policy question is whether sweeping improvements to the safety net can reduce the rate of violence against children. Indeed, numerous studies have shown in experimental and quasi-experimental contexts that an increase in income through cash assistance or tax credit programs reduces the odds of maltreatment (Berger, Font, Slack, & Waldfogel, 2016; Cancian, Yang, & Slack, 2013; Paxson & Waldfogel, 1999a, 2002, 2003b; Raissian & Bullinger, 2017; Wildeman & Fallesen, 2017). However, hardships do not occur in a vacuum. Multiple studies have shown that one form of hardship often proceeds another, resulting in cumulative and pronounced disadvantage. This explains why the most successful prevention programs – home visiting programs – target multiple hardships at once by heaping parenting interventions upon social interventions, connecting the highest-risk families with additional resources, income, and skillsets (García, Heckman, Leaf, & Prados, 2016; Olds, 2006). Yet, this form of intervention may not be appropriate for the majority of at-risk families with fewer, more tractable hardships.

Medicaid is a similarly holistic safety net program with evidence of important improvements to recipients beyond physical health. Previous studies have documented positive effects on recipients' medical expenses and medical out-of-pocket spending (MOOP), improved mental health, and reductions in payday loans, suggesting that access to medical care might be a stabilizing source in impoverished households (Allen, Swanson, Wang, & Gross, 2017; Boudreaux, Gonzales, & Saloner, 2017; Hu, Kaestner, Mazumder, Miller, & Wong, 2016; Remler, Korenman, & Hyson, 2017; Wherry, Kenney, & Sommers, 2016b). Seventeen states have yet to expand Medicaid and several expansion states are presently considering a retraction of benefits through either imposing work requirements or changing benefit levels. Many of the benefits of Medicaid are not fully understood, so a retraction of benefits (or failure to expand) could have negative externalities not accounted for by benefit-cost analysis. By one estimate, retraction of Medicaid benefits under the Patient Protection and Affordable Care Act (ACA) would result in a loss of coverage for 2.8 million people with a substance use disorder, and 1.2 million with a serious mental health disorder, two of the most prominent risk factors for perpetrators of child maltreatment (Frank & Glied, 2017). As medical reporters are the most prominent reporting source for infants – accounting for nearly one quarter of all reports for children under 12 months (see figure 1-1) – early prevention hinges on access to medical care. Given the ancillary evidence for Medicaid ameliorating the many contextual factors that put children at high risk for maltreatment, quantifying the direct benefits of Medicaid in a quasi-experimental framework is an important policy imperative.

In this paper, I provide the first evidence on the effect of Medicaid on young child maltreatment rates using the exogenous rollout of California's Low Income Health Program (LIHP). Using detailed administrative data covering the census of CPS reports in 50 states from 2010 through 2013 from the National Child Abuse and Neglect Data System (NCANDS), I compare the child maltreatment rates among children age five and younger in expansion counties to those non-expansion counties within and outside of California. My findings broadly indicate that access to Medicaid indeed is associated with a reduction in child maltreatment, especially physical abuse among traditionally disadvantaged populations of young children.

This paper proceeds as follows. After a brief discussion of child maltreatment and the policies aligned to protect children, I describe the Medicaid expansion and the three potential mechanisms through which I expect Medicaid to impact maltreatment rates. Following a description of the five data sources, I describe my empirical approach and results. I employ a number of robustness checks – discussed in the following section – followed by a discussion and conclusion.

Child Maltreatment

Child maltreatment is broadly defined as "any recent act or failure to act on the part of a parent or caretaker which results in death, serious physical or emotional harm, sexual abuse or exploitation; or an act or failure to act, which presents an imminent risk of serious harm," according to the Child Abuse and Treatment Act of 1988 (CAPTA), reauthorized in 2010 (P.L. 111-320). As states and localities have the autonomy to further define child protection laws and employ the necessary enforcement, there is ample cross-state variation in the criteria for investigation and punishment. States tend to vary with respect to funding and the intensity of their responses as well, though there has been a general shift towards either keeping children in the home or placing them with relatives as opposed to non-relative foster care under Title IV-E of the Social Security Act.[2]

Though there is some causal evidence that maltreatment is harmful to children's short- and long-term outcomes, the effect of maltreatment is difficult to disentangle from other factors such as family and neighborhood environments, socioeconomic disadvantage, prenatal and postnatal health investments, etc. that might differentiate maltreated children from those who are not maltreated. Thus, limited data availability and the absence of randomized studies robust causal

[2] https://www.childwelfare.gov/pubPDFs/kinship.pdf

inference in the majority of studies. Although attention has long been paid to the consequences of maltreatment across literature within the relevant disciplines, causal studies are rare for these reasons. Even within the studies that are closer to providing a well-identified effect of maltreatment, vast underreporting implies that the calculated effects may overestimate the true effects of maltreatment as only the more severe cases are detected and investigated and ultimately quantified (Currie & Spatz Widom, 2010).

In the early health and development literature, child maltreatment can be theoretically conceptualized both as a shock to early health endowment and as a negative parental investment. For instance, prenatal maltreatment could result in differences in health at birth, as indicated by conditions such as Fetal Alcohol Syndrome (FAS) or Neonatal Abstinence Syndrome (NAS), the prevalence of which has markedly increased due to the opioid crisis. Similarly, child maltreatment can be perceived as a negative parental investment – in other words, that which occurs after birth. Conceived this way, maltreatment affects children across multiple domains of well-being; in the short-term, it has been linked to poorer outcomes in education, cognitive ability and employment, physical and mental health, and adverse behaviors. Two quasi-experimental studies showed that maltreated children showed large and significant deficits in IQ, reading scores, and school performance (Currie & Widom, 2010a; Perez & Widom, 1994). Though the cumulative effects of maltreatment are scarcely known or studied, both studies found that the effects persisted into adulthood with the latter study finding a 14 percentage point gap employment and an $8,000 gap in earnings. Maltreatment has also been associated with a host of mental health outcomes, including internalizing behaviors such as anxiety and depression, externalizing behaviors such as aggression and perhaps not surprisingly, nearly all forms of childhood maltreatment are correlated with PTSD (see Gilbert et al., 2009 for a complete review). Finally, maltreatment has also been linked with aggression, violence, and criminality through the lifespan (Currie & Tekin, 2006, 2012).

Policy Background

In 2010, California adopted an early Medicaid expansion program, the Low-Income Health Program (LIHP), using its "Bridge to Reform" §1115 Medicaid Demonstration Waiver matched at 50 percent with the Patient Protection and Affordable Care Act's (ACA) early expansion funding. LIHP expanded Medicaid eligibility up to 200 percent of the Federal Poverty Level (FPL), though counties enacted thresholds independently ranging from 138 percent to 200 percent FPL. After January 2014, participants were auto-enrolled in Medi-Cal, California's Medicaid program (or moved into marketplace insurance due to a change in eligibility status).

Numerous studies have documented enrollment increases following California's Medicaid expansion. Parents and adults who were previously ineligible enrolled at rates 30 percent higher compared to the pre-expansion rates. Although children were previously eligible for insurance coverage through CHIP up to 200 percent of the FPL, a handful of studies have documented boosts in children's enrollments following an increase in the parental eligibility threshold (Hudson & Moriya, 2017; Ku & Broaddus, 2006). There is ample evidence of a 'first stage' effect of Medicaid expansions. In randomized control trials, the Medicaid expansion has been causally linked to an increase in healthcare utilization, an improvement in users' self-reported overall health, and to a 30 percent decline in depression (Baicker et al., 2013a; Finkelstein et al., 2012). Later quasi-experimental studies found that these improvements were partially explained through reductions in out-of-pocket medical spending, a reduction in medical debt, and reduced poverty rates (Allen et al., 2017; Boudreaux et al., 2017; Hu et al., 2016; Remler et al., 2017; Wherry et al., 2016b).

Access to Medicaid can benefit children in four primary ways. First, there is ample evidence that access to Medicaid improves parental health care utilization and mental health (Baicker et al., 2013b; Finkelstein et al., 2012). A healthier parent is more likely to work, is less prone to stress, and may be more likely to use and benefit from mental health services (Currie & Madrian, 2000). Medicaid coverage for substance addiction treatment and the many physical and mental health ailments known to afflict maltreating parents could mechanically improve the behavioral aspects of maltreatment as well.

Second, access to Medicaid might benefit children through the contemporaneous increase in household income or a positive change in the household budget. Prior evidence has linked Medicaid access to reduced out-of-pocket medical expenditures (MOOP), a reduction in payday lending, a reduction in overall medical debt (Allen et al., 2017; Baicker et al., 2013b; Finkelstein et al., 2012; Hu et al., 2016) and lower poverty rates (Wherry, Kenney, & Sommers, 2016a). Children might benefit directly through increased investment as well as through a reduction in household stress. These may be important mechanisms given the established relationship between household income and child maltreatment rates. In a series of path-breaking studies using state-year aggregated data, Paxson and Waldfogel (1999; 2002; 2003) find ample evidence of a relationship between income, poverty and maltreatment. Their findings suggest that a 1-percentage point increase in the fraction of children below 75 percent of the poverty threshold is associated with a 3.8 percent increase in the number of maltreatment cases. A series of randomized control trials found that a boost in annual income decreased maltreatment rates (Cancian et al., 2013; Wildeman & Fallesen, 2017), and a recent study found that the increase in family income by about $1,000 annually from the EITC resulted in a reduction in neglect by 3 – 4 percent and a reduction of CPS reports by 8 – 10 percent among low-income, single mother families (Berger et al., 2016).

Access to Medicaid has consistently been found to directly increase healthcare utilization among children (see E. M. Howell and Kenney 2012). Increasing the eligibility threshold of parents appears to boost child enrollments as well (Hudson & Moriya, 2017; Ku & Broaddus, 2006). Medicaid has been found to reduce avoidable hospitalizations and infant/child mortality, two measures that may be correlated with maltreatment rates (Aizer, 2007; Bermudez & Baker, 2005; Bhatt & Beck-Sagué, 2018; Currie & Gruber, 1996; E. Howell, Decker, Hogan, Yemane, & Foster, 2010; Kaestner, Joyce, & Racine, 2001). Medicaid-spurred improvement in infant and child mortality rates might eventually shift the average population health of children through improved maternal health and prenatal care, leading to a decline in the number of children born with developmental disabilities and other limiting conditions linked to abuse and neglect. Furthermore, as pediatricians are a trusted resource for parents regarding child development and discipline strategies, physician access may directly improve parenting practices as well (Bass et al., 1993; Combs-Orme, Holden Nixon, & Herrod, 2011; Flaherty & Stirling, 2010; MacPhee, 1984; Regalado, Sareen, Inkelas, Wissow, & Halfon, 2004; Taylor, Moeller, Hamvas, & Rice, 2013).

Finally, Medicaid could also directly benefit children through increased time with mandated reporters. If mandated reporting among physicians falls after a child reaches their first birthday, it could be that increasing face time with physicians could boost reporting among children 1 – 5. Further, the ACA allocations for home visiting programs could prevent long-term child maltreatment, benefiting children throughout their lifetime. This latter mechanism could conversely result in an increase in reported maltreatment. As physicians are a primary mandated reporting source for young children, increased exposure could increase reporting rates. I consider and test for this possibility, as discussed in the empirical methodology section.

Data

My primary data includes the census of child maltreatment reports for children under six years in the National Child Abuse and Neglect Data System (NCANDS) from 2010 through 2013. These data are collected biannually and administered by the National Data Archive on Child Abuse and Neglect housed at Cornell University. Though contribution is voluntary under the Child Abuse Prevention and Treatment Act of 1988, NCANDS has become the primary source for child maltreatment statistics in the United States. From 2010 through 2016, 51 states contributed data to NCANDS, all of which are included here with the exception of Puerto Rico, though I limit my primary estimations to the three-year county-month sample from January 1, 2010 through December 31, 2013 for a total of (N= 4,755,579) children aggregated to N=33,073 county-months. There is substantial variation in report rates across county, as illustrated in the kernel density curve in figure 1-2.

I use the 2010 Small Area Health Insurance Estimates (SAHIE) from the U.S. Census Bureau to estimate the pre-expansion, county-level insurance rates. Unlike estimates from the American Community Survey, these estimates include counties with populations under 65,000 and incorporate Medicaid enrollment rates to provide the most accurate county-level rates available. The 2010 Small Area Income and Poverty Estimates (SAIPE) provide the weighted and adjusted county-level poverty rates. I use the overall poverty rates, rather than that of children, to account for potential changes in parental eligibility. County population data comes from the National

Cancer Institute's Surveillance, Epidemiology, and End Results Program (SEER) for the entire period of inquiry. I compile age-specific population estimates into a single estimate for the population of children under six. County-level unemployment rates come from the Bureau of Labor Statistics Local Area Unemployment Statistics (LAUS) for the same period.

The many ways that previous authors have measured child maltreatment require distinction; maltreatment is an overall measure of child abuse and neglect and can be captured either as a behavioral approximation (see e.g. (Berger et al., 2016)), or as a count of screened-in maltreatment reports (Cancian et al., 2013). Though the latter is notoriously prone to underreporting and the former yields a rough approximation of overall maltreatment, both are commonly used to measure overall maltreatment rates in a population. Some studies measure CPS involvement with parental self-reports as well, however these are also prone to bias. It is common to separate neglect and abuse reports to detect treatment heterogeneity, which assists in understanding the pathways between poverty and maltreatment, as neglect -- which is often characterized by being in a state of resource scarcity -- may be more closely tied to income and economic disadvantage than other forms of maltreatment. Finally, several studies use Out of Home care (OOH) rates as an approximation for abuse and neglect. While typically borne out of data limitations, findings from these studies must be interpreted with caution, because differences in state policies, reporting mandates, and intervention methods (i.e. dual track responses) would skew the true distribution of child maltreatment rates.

Accordingly, I use four primary outcome measures which are later disaggregated into maltreatment type. The first captures the raw CPS report rate per 100,000 children. Though this variable is right skewed, I include it for comparison with previous literature alongside a log-transformed variable. In addition, I include two indicators for whether the alleged maltreatment was substantiated and for whether the child was removed from the home and placed into foster care. As both of these may reflect substantive state-, county-, or agency-level differences in policy, the log of CPS reports provides the most robust measure.

I use the American Community Survey from 2007 through 2013 for robustness checks, approximating maltreatment three ways. First, I construct a variable indicating whether a child is living without both parents, then iterate this variable as an indicator for whether the child and siblings are living without both parents. My third measure indicates whether a grandparent is caring for the child.

Empirical Approach

To estimate the causal effect of access to Medicaid on child maltreatment, I require a plausibly exogenous source of variation in Medicaid. If access to Medicaid is random (or quasi-random, as proposed here), the observed outcomes in an OLS model are more likely to be the result of differential access to Medicaid, as opposed to some other driving force. A conventional OLS model would be prone to bias due to selection into Medicaid and the presence of other unobserved factors that might contemporaneously predict Medicaid participation and maltreating behaviors. For instance, disabled or otherwise disadvantaged parents may be more likely to enroll in Medicaid and to maltreat their children. As disability status is unobserved, the effect of Medicaid would be biased away from zero. Therefore, I identify the effect of Medicaid using the temporal and spatial variation in the early California Medicaid expansion. Using a difference-in-difference framework (DD), I compare the pre- and post- expansion abuse and neglect rates of children living in expansion counties to those in non-expansion counties, as characterized by equation 1 below.

$$(1)\ maltreat_{cm} = \alpha + \beta C_c + \rho M_m + \gamma X_{cm} + \theta POST_{cm} + \varepsilon_{cm}$$

Maltreatment outcomes ($maltreat_{cm}$) for children in county c in month m are measured as the number of reports per 100,000 children, are regressed on county- and month-fixed effects (C and M, respectively), a vector of controls for child and family characteristics (X), and an indicator, POST, set to 1 for California counties beginning in the month of their expansion and thereafter, and 0 for all counties outside of California and those within California prior to expansion. The coefficient of interest θ represents the estimated intent-to-treat (ITT) effect of Medicaid on child maltreatment reports. County-level fixed effects account for static differences across counties and month effects account for temporal changes that affect all counties uniformly. County-specific linear time trends are included in later models to account for other factors that vary across counties over time, such as compositional shifts in Medicaid-eligible populations or differential responses to the Great Recession. All regressions are executed using OLS models with White robust standard errors clustered at the county level and weighted by the county population of children younger than six years of age.

A key assumption underlying the DD methodology is that absent the Medicaid expansion, the trends in maltreatment would have been indistinguishable across treated and untreated counties. Though this assumption – often referred to as the parallel trends assumption -- is inherently untestable, I explore the presence of pre-trends by regressing a trend variable with county-level indicators among the pre-treatment sample, as shown in equation (2) below.

$$(2)\ maltreat_{cm} = \alpha + \gamma X_{cm} + \beta TR_{cm} + \theta TREAT_{cm} + \rho TRxTREAT_{cm} + \varepsilon_{cm}$$

$TREAT$ indicates treatment status, equal to 1 for California counties with expanded Medicaid and 0 otherwise. TR is the linear time trends, and ρ is the coefficient of interest, indicating whether the pre-treatment trend differs between the treated and control counties.

Omitted variable bias is another common threat to causality, where other unobserved factors that predict maltreatment are spuriously related to the error term. Accordingly, the key identifying assumption for causal inference is that access to Medicaid did not plausibly change the demographic characteristics of children prone to maltreatment, nor other factors that might affect reporting, especially among Medical personnel. To test the first part of this assumption, I regress the child demographics on treatment status.

With regard to the latter assumption, medical personnel have been designated mandated reporters in California since 1963, though at the time only physicians were noted and were responsible only for reporting physical abuse. California's Child Abuse and Neglect Reporting Act (CANRA) of 1980, § 11165.7 part (21) expanded the list of responsible medical personnel to include the myriad of reporters mandated today. Two sections of CANRA were enacted in 2011 or 2012, neither of which would have had any bearing on reporting behavior. The first, § 11167 part (f) outlined the type of information collected on a report and the confidentiality rules for reporters. The second, clarified the definitions of substantiation, unfoundedness, and inconclusiveness (§ 1165.12).[3]

To ensure exact counterfactuals and to account for potential bias from omitted variables, I also estimate synthetic control models. This method ensures that the most precise counterfactual counties are used by creating a synthetic treated county based on the characteristics and pre-law trends in the outcome variable. The resulting synthetic county is a weighted combination of other

[3]https://leginfo.legislature.ca.gov/faces/codes_displayText.xhtml?lawCode=PEN&division=&title=1.&part=4.&chapter=2.&article=2.5

untreated counties. The weights are selected such that the difference in the characteristics in the treated counties and the other counties is minimized. The weights sum to one and are estimated separately for each county. I employ the cross-validation method for selecting predictor weights (Abadie, Diamond, & Hainmueller, 2015).

Results

Table 1-1 shows the summary statistics. The children in the census of maltreatment reports have a mean age of 2.43 and are predominantly white. The vast majority of reports were unsubstantiated, and only 14 percent of reported children were eventually removed from their home and placed into foster care. In line with previous studies, 61.71 percent of the sample is comprised of children who were alleged victims of neglect and nearly 16 percent of physical abuse. Around 28 percent of the children's caregivers reported having financial hardships at the time of the report, with an even greater percentage reporting receiving any type of public assistance. The majority of the children hail from unknown family structures at the time of the report. Of those for whom the family structure is known, cohabiting is the most common.

Table 1-2 shows the primary results from DD estimations. In column 1 of panel A, the expansion is credited with 35 fewer reports per 100,000 children in a given county-month, a reduction of nearly 4 percent relative to the pre-expansion mean. When county-specific linear time trends are included, the effect falls to 10 reports per 100,000 children. Though insignificant, the direction and magnitude suggest that the effect is not negligible, and as I discuss later, is likely masked by competing heterogeneous effects. Turning to the reports disaggregated by maltreatment type, access to Medicaid drives down reports of physical abuse by 7.98 reports per 100,000 children per county-month and sexual abuse reports by 1.68 reports per 100,000 children. Panels C and D show the estimates from the same model using the log of the reports as an outcome measure. As this variable is normally distributed, I consider these estimates less prone to bias and considerably more reliable. Access to Medicaid appears to reduce Physical abuse CPS reports by 11 percent (panel D, column 2), representing the intent-to-treat effect (ITT), with similarly negligible effects on all other outcomes with one exception. There appears to be a rather large, negative effect on other types of maltreatment (column 6), with my preferred estimates suggesting a significant 78 percent decline in the number of maltreatment reports alleging an 'other' type of maltreatment.

In order to estimate the effect of the treatment-on-the-treated (TOT), I estimate the same model with a subset of counties for whom the exact compliance rate is available (appendix table 1-A2). The estimated average compliance rate in this set of counties was 28.5 percent as of 12/2012, yielding an implied TOT of 42 percent.[4] Though not all CMSP counties are included in my primary sample, the average compliance rate among these counties is 28.8 percent,[5] resulting in a TOT of 39 percent. In other words, for every 12,607 new enrollments, physical abuse CPS reports declined by 1 percent.

Due to state- and county-level differences in defining and prosecuting child abuse and neglect, the raw CPS report rate is widely considered the best measure of maltreatment. However, prior studies assessing child maltreatment prevention strategies often use the rate of substantiated reports and removal (foster care rate) as proxy measures. Accordingly, table 1-1 through 1-3

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[4] ITT = .12 (column 2 panel B in table 1-A2), compliance = .28. For TOT = ITT/ compliance, the implied TOT = .42.

[5] An estimated 506,660 of the 1,757,000 eligible individuals enrolled = 28.52 % for CA

presents the same models as in table 1-2, using these two alternative measures. Note that the rate of substantiated reports appears unaffected in the full model (panel B column 1) and in the maltreatment subtype models. Interestingly, the foster care rate falls in most specifications (panel D). This could be an artifact of parental access to Medicaid, which may have resolved any tractable parenting issues that otherwise would have kept children in care. For instance, children whose parents were addicted to opioids would have gained access to treatment under the expansion, along with a laundry list of other physical and health benefits.

I next probe whether my primary effects are being driven by county size. In table 1-4, I omit the weights and stratify three primary outcomes by an indicator for whether the observed county is above or below the median observed county population (using exclusively the log of the report rate). If small cell sizes were driving these effects, we would expect the small counties to have inflated coefficients. Fortunately, this is not the case, as seen when comparing columns 1-3 to 4-5. In panels B and C, I present the results from models stratified by county poverty rates (B) and 2010 insurance rates (C). I delimit counties in the same way, coding an indicator with "1" if the county is at or above the observed median and "0" if below. If these results are in fact being driven by the Medicaid expansion, we would expect to see larger effects in more disadvantaged counties (panels B and C, columns 4, 5, and 6). My results generally support this assertion, with the exception of all reports having a larger, significant negative effect in counties with high insurance rates (column 1).

Effect Heterogeneity

Prior evidence suggests that a retraction of Medicaid benefits could be particularly detrimental so historically underrepresented subgroups of the population. Under previous expansions, Medicaid was accredited for reducing maternal mortality and infant mortality, increasing overall access to care and healthcare utilization, and reducing the general burden of medical debt. To that end, in this section, I further disaggregate by child characteristics and other markers of disadvantage. For the remainder of the paper, I retain only my preferred specifications with log outcomes and county-specific linear time trends.

Table 1-5 presents the results from equation (1) on the full sample for major maltreatment type, substantiation, and foster care disaggregated by child race/ethnicity. Though the effects for Black and Other children are relatively larger, up to 19 percent, the effect for White children is relatively large as well. The foster care rate follows the same general pattern, though the strongest effects appear among White children. These results suggest that children of traditionally disadvantaged race/ethnicities face a significant increase of physical abuse by up to 19 percent and around a 2 percent increase in foster care in the counterfactual world without Medicaid. The smaller effects among Hispanic children could be due to the fact that enrollment is conditional upon citizenship, a question verifiable in future research with CPS reports and insurance status in the same data.

As newborns were auto-enrolled in SCHIP for their first year, it is unsurprising that the effect among infants (< 12 months) is relatively small and insignificant. In California, the income eligibility limits for infants was higher (200% FPL) compared to that of older children, so this age group in particular would only directly benefit from the expansion by enrollment of older siblings, parents, and relatives.[6] Most other ages appear to benefit, though the one year olds to a much greater degree. All age groups see a decline in foster care rates, following the same pattern as the results in table 1-2. In table 1-7, the same models are disaggregated by child gender. Though both genders benefit from Medicaid in terms of significantly reduced physical abuse, females benefit more than males, by nearly 8 percentage points (14 percent relative to six percent). The foster care benefits are evident in these models as well.

At the time of the CPS report, the child's primary caregiver was asked whether they relied on any type of public assistance. Table 1-8 shows the same DD models stratified by this indicator. Families who answered 'yes' saw a 25 percent reduction in all reports, a 28 percent reduction in physical abuse and a 20 percent reduction in neglect, all significant at 95 percent or higher. Though the results for physical abuse among the more advantaged families retain significance, the magnitude of the effect is nearly half that as the disadvantaged families. Not only do these results give further support to the notion that these effects are being driven by Medicaid, but they reveal a potential direct pathway through which children might benefit – if families are on public assistance with and without Medicaid, then these effects can be attributed to Medicaid alone, as the sole source of variation in these effects. Unlike the previous models, children from disadvantaged families see a small but significant reduction in neglect, suggesting that Medicaid may curtail maltreatment by relaying additional resources and benefits that the absence of which would be classified as neglect.

[6] Repeating this analysis without infants yields the same results (available upon request).

If children gain access to Medicaid through their caregivers, the surveillance hypothesis posits that increased time with medical personnel resulting from increased medical care access and usage can result in an increase in reports. To test this assertion, I stratify the same models by an indicator for whether the reporter was medical or non-medical, as shown in table 1-9. The effect on medical reporters is negative, undermining the surveillance hypothesis. However, it is important to note that if the decrease in reports by medical personnel is offset by an increase in non-medical personnel, the spillover effects of the law could only be interpreted as a substitution effect away from medical reporters, rather than an actual change in child maltreatment. Conversely, reports fall among both groups of reporters, giving further credence to an absolute reduction in maltreatment.

Robustness Checks

To further probe the robustness of these results, I turn to the American Community Survey (ACS) approximating for child maltreatment with an indicator for whether a child was living out of the home at the time of the survey in the same time period. As the ACS has more detailed information on household and family characteristics, I similarly specify the sample, retaining only children under six years of age from 2010 through 2013. As the child's maltreatment status is unobservable (as in most major household surveys), I construct an indicator for whether the child is living with either parent, so that children living without a parent is equal to one, and zero otherwise. Parallel measures in the ACS indicate whether all children in the household are living without parents, and whether children are living with grandparents. These are only included as subsidiary outcomes as they exclude children living with other kin or relatives.

I employ the same difference-in-difference model as in equation (1), shown in panel A of table 1-10, and include county-specific linear time trends as shown in panel B. Though the sample is markedly larger, the effects follow the same general pattern as my primary data, albeit at diminished significance. Children are .04 percentage points less likely to be living without their parents (25.96 percent of the pre-treatment mean) and .03 percentage points less likely to be living with a grandparent (both significant at 10 percent). However, the effects are considerably larger, up to 26 percent of the pre-expansion mean, suggesting that there is likely some additional heterogeneity not captured by these specifications. These results give further credence to my primary findings.

If access to Medicaid is the source of reduction in child maltreatment reports, we would expect to see a similar pattern from the 2014 ACA expansion. Though the rollout was at the state level, 26 states enacted an expansion January 1, 2014, with another five in the following five years. The results, shown in table 1-11, follow the same general pattern, though in the preferred specification with state-specific linear time trends, only the coefficient on the number of reports remains marginally significant. This could be because uptake rates are not observable in these data, and because other, related policies resulted in a surge of funding in the same period, degrading the variation needed for identification. Further, when I restrict the sample to California alone, the same pattern emerges, albeit with much less power (panel B).

One further possibility that would undermine the DD approach would be if caregivers manipulated their treatment assignment – for example, moving into a county to become Medicaid-eligible –resulting in an effect that captures both a shift in the eligible population and the effect of access to Medicaid. To rule out this possibility, I regress on the treatment indicator a set of child characteristics. These results (available upon request) suggest that this possibility is weak at best, with marginal significance in one indicator for children's age. Overall, there is no evidence to suggest that treatment group manipulation is problematic.

If pre-trends in the outcome variables vary between Medicaid expansion and non-expansion counties, the credibility of these results would decline, as the effect could be biased by factors other than Medicaid. To test this assumption, I estimate equation (2) on the pre-treatment sample, shown in table 1-12. The first row indicates that there is no significant difference between the outcomes across the top in the treated and untreated counties, implying that the parallel trends assumption is met. However, the results from the dynamic policy effects model I estimate in the following section suggest that this assumption may be violated, urging a conservative interpretation of the results on all reports and neglect reports. Conversely, physical abuse reports appear to be entirely free of pre-trends, giving further credence to the reliability of my estimates.

Another threat to validity is the theoretical construction of a control group. If the control group fails to represent a perfect counterfactual, the observed differences in report rates could be biased away from zero, capturing other nuances in unobserved heterogeneity. To test this possibility, I employ a synthetic control estimation, averaging across the county treatment effects, the difference between California and synthetic California, to generate figure 1-2 using a lowess estimator. The vertical dashed line represents the average month in which Medicaid was expanded across all California counties. The treatment effect for all CPS reports and neglect reports is positive in the pre-expansion period, followed by negligible effects in the post-expansion period. This pattern suggests that Medicaid may have been expanded in counties with relatively high CPS reports pre-expansion. Alternatively, the null effects in the pre-expansion period followed by negative effects post-expansion for physical abuse reports follows the pattern that emerges from my primary results. Comparing the estimated treatment effects to the reported compliance rate in figure 1-3, it is clear that high-compliance counties have relatively larger estimated treatment effects in all three outcomes.

As one final robustness check, I test to see whether the variation produced by the counties who expanded in the two primary expansion periods (7/2011 and 1/2012) is adequate to produce the same results. To test this, I aggregate the data to the county subtype (those who expanded in each of two periods as well as non-expansion states) x six-month-block x maltreatment type and employ the same DD design. The raw plot of CPS reports (figure 1-A1 in the appendix) reveals the pattern we would expect, with a drop in abuse reports following each expansion. The DD test yields the expected coefficients, however the effect is much larger for all reports than for abuse reports (table 1-A3). Note that because all other non-expansion counties are in a single group, this check is less robust than the synthetic control estimation (results shown in figure 1-2). Unlike this check, the control counties include only those deemed to be statistically similar to each treated county, such that the control group is a more plausible counterfactual.

Dynamic policy effects

If California's early Medicaid expansions were truly exogenous, we would expect to see little or no discernable change in maltreatment reports before the expansion, and a large, cumulative effect after the expansion. To test this assertion, I implement an event study model, where I allow individual three-month period to enter the model as leads and lags, rather than the post indicator in equation (1). Accordingly, I estimate the following equation:

$$(3)\ maltreat_{cm} = \alpha + \beta C_c + \rho M_m + \gamma X_{cm} + \sum_{j \in J} \theta_j POST_{cm}^{j} + \varepsilon_{cm}$$

Where $POST_{cm}^{j}$ is a series of dummy variables equal to 1 for the counties in which the expansion was in place for j periods, J ={-12, -9, -6, -3, 0, 3, 6, 9, 12} with the three-month of the expansion as the omitted category (0). I estimate models with and without county-specific linear time trends, though only report the latter as these represent my more conservative estimates.

The results for all CPS reports are shown in figure 1-4. The top panel shows the coefficients for the full sample, followed by physical abuse reports only in the center panel and neglect reports in the last panel. The results in the full sample (top panel) follow the same pattern as the synthetic controls, suggesting that the models with county-specific linear time trends are the most robust, as these are more likely to account for any unobserved heterogeneity. In accordance with the DD model findings, Medicaid appears to have the largest effect on physical abuse rates (bottom panel), especially in the months immediately after the expansion.

Discussion and Conclusion

This study provides the first evidence that access to Medicaid may reduce violence against children. Motivated by the evidence linking the early childhood environment with lifelong health and human capital production, early access to Medicaid might ameliorate the economic, cognitive, and mental and physical health disparities associated with childhood abuse and neglect (Berger & Waldfogel, 2011; Currie & Tekin, 2006; Currie & Widom, 2010a; Gilbert et al., 2009; Perez & Widom, 1994; Robinson et al., 2012). I identify the effect of access to Medicaid by the quasi-experimental variation due to California's county-level Low Income Health Program in 2011 – 2012. I find that children experienced an 11 percent reduction in reports alleging physical abuse, though these effects are notably larger among more disadvantaged children. Children living in homes with financial hardships see a 28 percent reduction in maltreatment reports, and those who are either Black or Other race/ethnicities benefit by up to 19 percent of the pre-expansion mean. My results are robust to synthetic controls analysis and alternative data samples and specifications.

As Medicaid under ACA was not designed to prevent child abuse, these striking findings inform Medicaid's overall cost-benefit calculation and provide a new strategy for practitioners and legislators seeking to reduce maltreatment and the costs thereof. Though the potential mechanisms remain theoretical, ancillary evidence points to multiple, cumulative facets of disadvantage that may result in a reduction of maltreatment rates, including stability of the household budget, a reduction in stress and depressive symptoms, and an overall improvement in parental and child health. Future researchers should identify ways to test these mechanisms using data that capture both child maltreatment status and insurance status. Though there is a great deal of evidence pointing to income effects, per se, little is known about the cumulative effects of combining income supplementation with insurance, or other holistic improvements to the parenting context. Home visiting programs provide perhaps the best evidence of this type, though due to their high relative cost and general preference for families known to CPS, the vast majority of at-risk children are ineligible for their benefits.

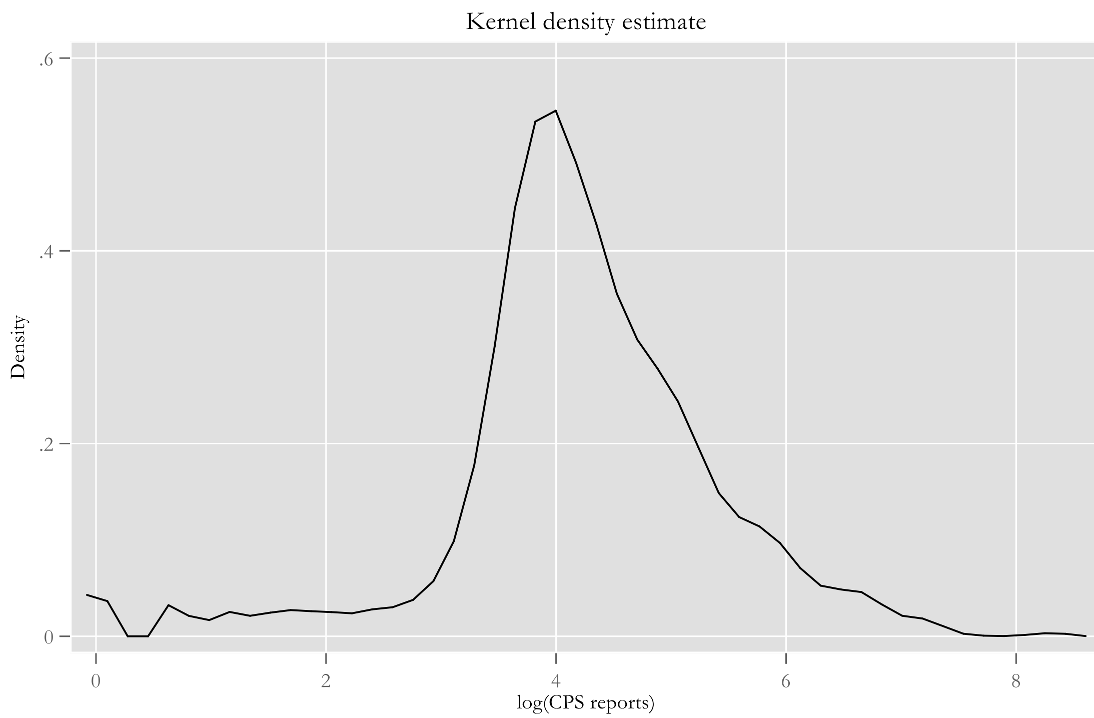
The effects I detect here are well within the range detected in previous studies, though the effects here are concentrated on physical maltreatment. Medicaid remits around $500 per year in terms of financial benefits in addition to the many non-financial benefits I discuss at length above, implying that my effects should align with those from both cash and in-kind programs. Several previous studies detect a reduction in child maltreatment due to safety net and other antipoverty programs. One such study used an existing randomized control trial to identify the effect of additional child support received on child maltreatment rates among a large sample of TANF recipient families in Wisconsin (Cancian et al., 2013). The additional child support was garnered through a full pass-through and disregard, causing an increase in annual family incomes by around $170 compared to the control groups. Though it appears small, the treatment group saw a 10 percent reduction in the odds of being screened-in for a child maltreatment investigation, representing a 2-percentage point reduction over a two-year period. A later study using Danish registry data found a similar effect, albeit with a much larger increase in income (Wildeman & Fallesen, 2017). Identified by a 2004 law that effectively lowered the welfare payment ceilings, the authors find that the resulting 30 percent reduction in income was associated with a 1.5 percentage point increase in the odds that a child was involved in CPS, or, a 25 percent increase in the annual risk of CPS involvement. The increase in income was equivalent to around $4,800 per year, yet the effect was similar in magnitude, suggesting that these results may not perfectly translate to the US context. Although both studies are limited in generalizability due to their sample of welfare recipients, the overall pattern of causal and observational studies is highly suggestive of a causal effect of income on child maltreatment (see also Berger and Waldfogel 2004; Slack, Lee, and Berger 2007). Paxson and Waldfogel (2002; 2003) find that welfare reform is correlated with child maltreatment as well; benefit reductions appear to increase caseloads, and various measures of generosity – benefit levels, lifetime limits, sanctions, work requirements -- appear to have a similar effect (more generous policies yield lower caseloads). These effects are also relatively large; a 10 percent increase in welfare benefits is associated with a reduction in OOH care by nearly 8 percent. The latter finding suggest that even if welfare reform had competing effects (improved the lives of some and diminished that of others), the net effect on child maltreatment was overall negative. That welfare reforms generated negative externalities in child maltreatment rates was the same conclusion drawn by later authors as well

(Wildeman & Fallesen, 2017). Other safety net programs appear to affect child maltreatment rates, presumably through the family budget as well. Although Paxson and Waldfogel (2003) failed to detect the effect of EITC state benefit levels on child maltreatment, a later study identified the effect of EITC generosity using an instrumental variable approach that takes advantage of the geographic and temporal variation in state EITC benefit structures (Berger et al., 2016). Drawn from a longitudinal sample of families from the Fragile Families and Child Wellbeing Study, the authors find that the EITC instrument caused a first-stage increase in family income by about $1,000 annually, resulting in a reduction in neglect by 3 – 4 percent and a reduction of CPS reports by 8 – 10 percent among low-income, single mother families. These results are robust to control function methods and to other samples, indicating that the permanent shifts in income experienced by these families substantially reduced neglect and CPS reports, but not physical abuse. Several studies found that other factors such as foreclosure rates (Berger et al., 2015; Frioux et al., 2014; Wood et al., 2012), minimum wage (Raissian & Bullinger, 2017), and gasoline prices (McLaughlin, 2017) appear to predict a reduction in maltreatment as well. The latter two effect sizes are particularly interesting; a $1 increase in the minimum wage appears to reduce neglect reports by 9.6 percent, and a $1 increase in gasoline prices increases the overall maltreatment rate by 6.42 percent.

Despite the growing evidence of the importance of reducing child maltreatment in early childhood, the large-scale strategies for doing so are not well understood outside of parenting interventions and home visiting programs. Additional information is needed on the precise factors that contribute to mandatory reporting, and the conditions under which children benefit the most. One key question from this analysis is why reports among medical personal plummet after a child’s first birthday, and why child care providers initiate such a small percentage of reports. Though with this evidence, expanding Medicaid access and eligibility may be a useful strategy for improving the early parenting environment and early child health.

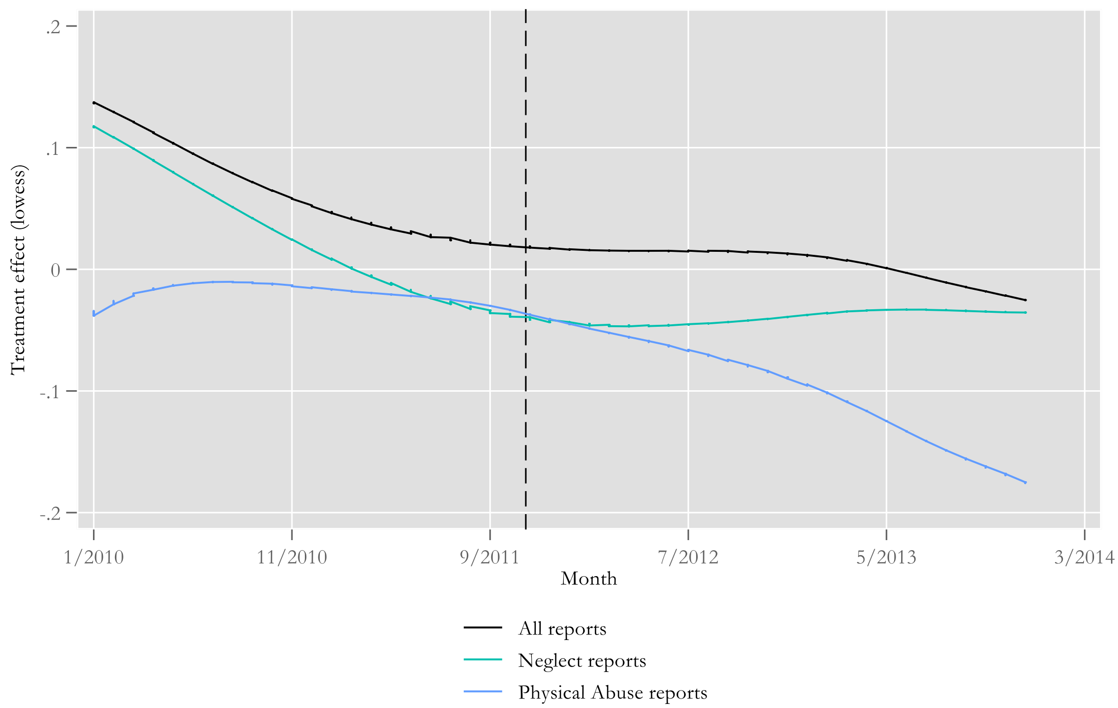
Figures

Figure 1-1: Variation in CPS Report rates by county



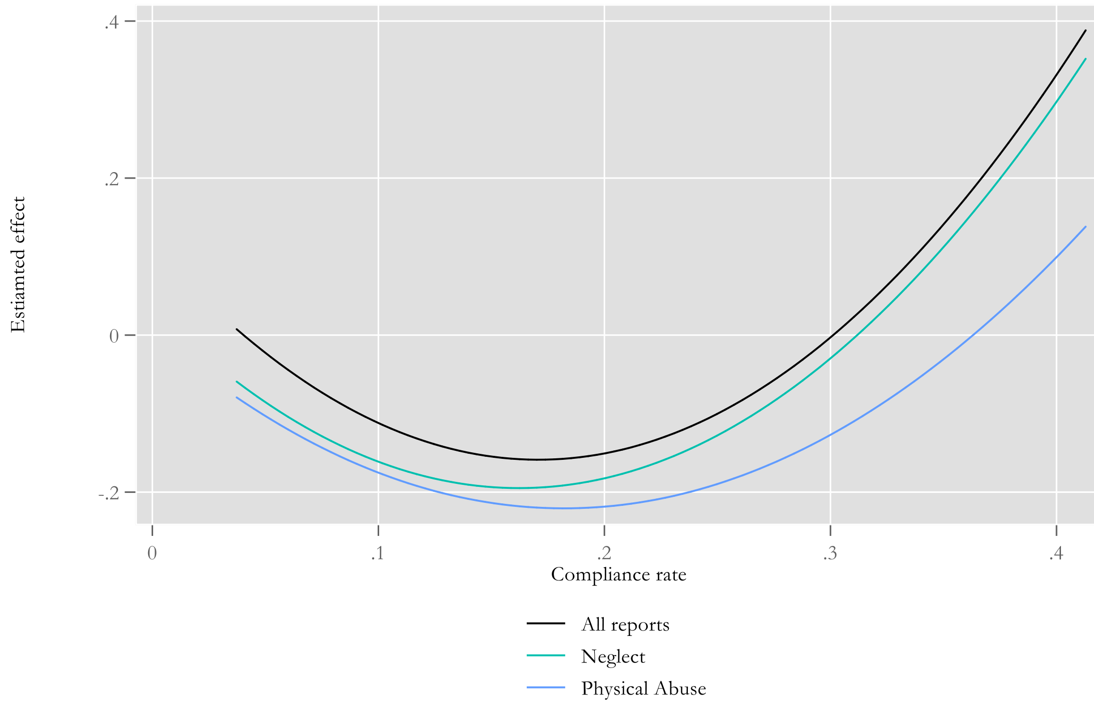
Note: Total alleged maltreatment reports to Child Protective Services (CPS) among children younger than six years from the census of reports drawn from 2010 – 2013 NCANDS administrative data.

Figure 1-2: Synthetic control effects (lowess smoothed)



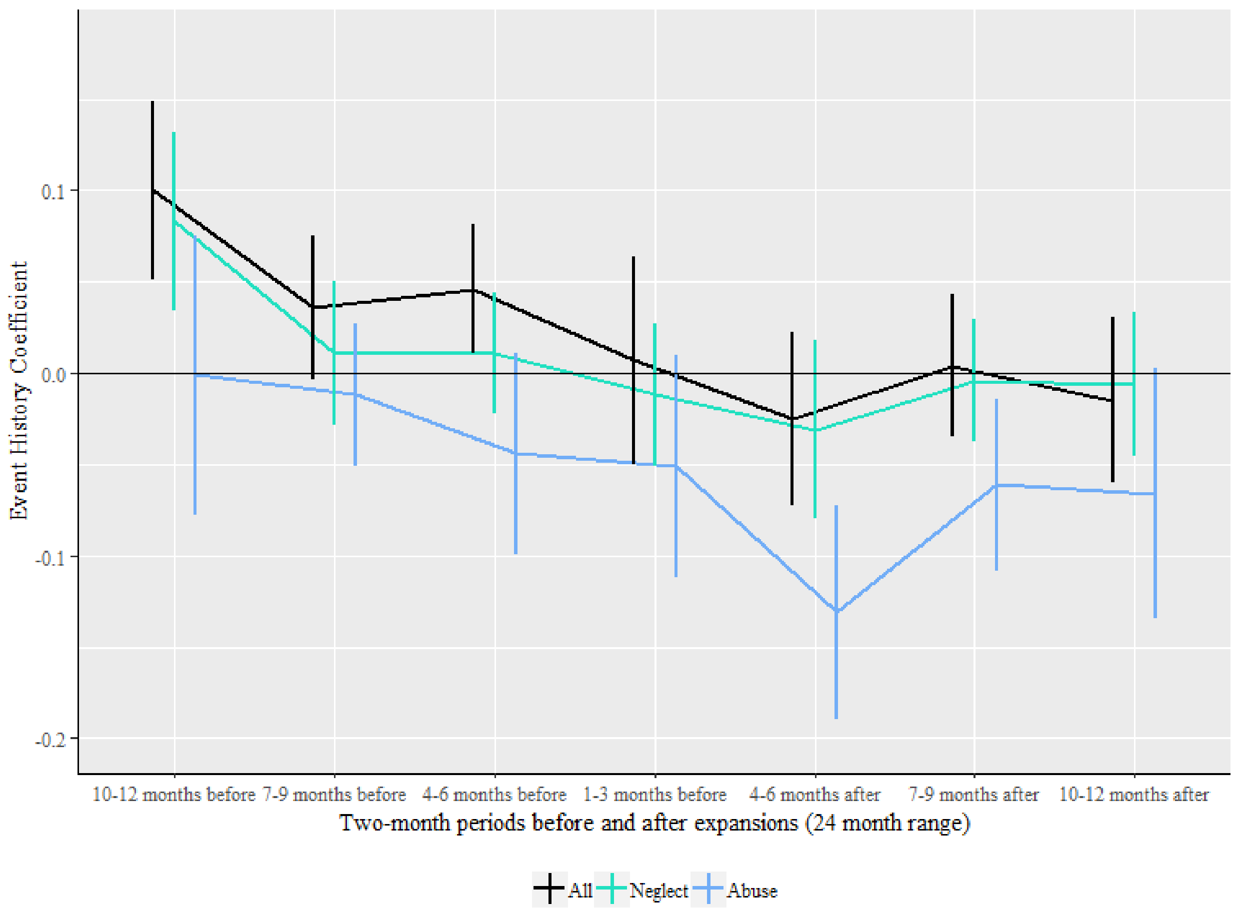
Note: Synthetic control models estimating the effect of Medicaid access on all reports, neglect reports, and physical abuse reports. Lines represent the average effects across counties using a lowess estimator. NCANDS administrative records of alleged child maltreatment reports 2010 – 2013 collapsed to the county level (N = 4,755,579). Covariates are included in the weighting stage for all models (county-level unemployment rate, county-level poverty rate, child race, child age, family structure).

Figure 1-3: Synthetic control estimates by compliance rate



Note: This figure shows the treatment effect by county-level compliance (uptake) rate. Synthetic control models estimating the effect of Medicaid access on CPS reports. NCANDS administrative records of alleged child maltreatment reports 2010 – 2013 collapsed to the county level (N = 4,755,579). Covariates are included in the weighting stage for all models (county-level unemployment rate, county-level poverty rate, child race, child age, family structure).

Figure 1-4: Dynamic policy effects



Note: Event history models are estimated using OLS weighted by the county population of children under six years of age. White robust standard errors are clustered at the county level. NCANDS administrative records of alleged child maltreatment reports 2010 – 2013 collapsed to the county level (N = 4,755,579). Covariates are included in all models (county-level unemployment rate, county-level poverty rate, child race, child age, family structure). LTT = linear time trends. State and month fixed effects are included as well. Vertical lines indicate 95 percent confidence intervals, *** p<0.01, ** p<0.05, * p<0.1.

Tables

Table 1-1: Descriptive Statistics

Full Sample		
	Mean/Freq	SD
Age	2.43	1.75
Race		
White	41.77%	
Black	27.84%	
Hispanic	23.64%	
Other	6.75%	
Foster care	0.14	0.35
Substantiated	0.24	0.43
Type		
Physical	15.84%	
Neglect	61.71%	
Sexual Abuse	3.68%	
Psych. Abuse	3.69%	
No Maltreatment	9.27%	
Other	5.80%	
Financial hardships	0.28	0.45
Public Assistance	0.32	0.47
Family Structure		
Married	7.32%	
Cohabiting	24.24%	
Single parent	18.13%	
Kin / OOH	2.86%	
Other / Unknown	47.45%	
N	4,755,579	

Table 1-2: Difference-in-Difference (DD) estimation

	(1)	(2)	(3)	(4)	(5)	(6)
	All reports	Physical Abuse	Neglect	Sexual Abuse	Psych. Abuse	Other
A: Rate per 100,000						
Treat*Post	-35.22**	-7.98**	-6.18	-1.68*	-1.84	-2.78
	(15.59)	(2.80)	(9.19)	(0.64)	(8.43)	(2.93)
r2	0.89	0.83	0.88	0.64	0.89	0.92
B: Rate per 100,000, with county-specific linear time trends						
Treat*Post	-10.23	-7.44***	-5.21	-1.35**	5.90	-1.53

	(7.04)	(1.47)	(6.17)	(0.42)	(3.77)	(3.11)
r2	0.91	0.86	0.90	0.67	0.92	0.93
C: Log(reports)						
Treat*Post	-0.09***	-0.10*	-0.01	-0.09*	-0.20	-0.26*
	(0.03)	(0.04)	(0.04)	(0.04)	(0.18)	(0.10)
r2	0.95	0.93	0.95	0.88	0.94	0.93
D: Log(reports), with county-specific linear time trends						
Treat*Post	-0.03	-0.11***	-0.01	-0.06	-0.02	-0.78***
	(0.02)	(0.03)	(0.03)	(0.04)	(0.07)	(0.09)
r2	0.97	0.94	0.97	0.88	0.95	0.95
N	33,073	31,284	32,691	26,123	10,583	8,643

Note: OLS models weighted by the county population of children under six years of age. White robust standard errors are clustered at the county level. NCANDS administrative records of alleged child maltreatment reports 2010 – 2013 collapsed to the county level (N = 4,755,579). Covariates are included in all models (county-level unemployment rate, county-level poverty rate, child race, child age, family structure). LTT = linear time trends. State and month fixed effects are included as well. Standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1.

Table 1-3: DD estimation, alternative measures

	(1)	(2)	(3)	(4)	(5)	(6)
	All reports	Physical Abuse	Neglect	Sexual Abuse	Psych. Abuse	Other
A: Log(reports), with county-LTT						
Treat*Post	-0.03	-0.11***	-0.01	-0.06	-0.02	-0.78***
	(0.02)	(0.03)	(0.03)	(0.04)	(0.07)	(0.09)
r2	0.97	0.94	0.97	0.88	0.95	0.95
N	33,073	31,284	32,691	26,123	10,583	8,643
B: Log(reports), with county-LTT and in CA, excluding CMSP counties						
Treat*Post	-0.02	-0.12***	0.00	-0.05	0.01	-0.78***
	(0.02)	(0.03)	(0.03)	(0.05)	(0.08)	(0.09)
r2	0.97	0.94	0.97	0.89	0.95	0.95
N	32,066	30,316	31,685	25,342	9,606	8,587

Note: OLS models weighted by the county population of children under six years of age. White robust standard errors are clustered at the county level. NCANDS administrative records of alleged child maltreatment reports 2010 – 2013 collapsed to the county level (N = 4,755,579). Covariates are included in all models (county-level unemployment rate, county-level poverty rate, child race, child age, family structure). State and month fixed effects are included as well. Standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1.

Table 1-4: DD estimation, stratified models

	(1)	(2)	(3)	(4)	(5)	(6)
	All reports	Physical reports	Neglect Reports	All reports	Physical reports	Neglect Reports
A: County Size						
	Above. Median County Pop.			Below Median County Pop.		
Treat*Post	-0.12***	-0.18***	-0.03	-0.57*	-0.15	-0.53
	(0.03)	(0.04)	(0.03)	0.32	(0.25)	(0.28)
r2	0.90	0.85	0.91	0.43	0.59	0.60
N	20,674	20,260	20,590	12,399	11,024	12,101
B: County Poverty Rates						
	Above Median Income			Below Median Income		
Treat*Post	-0.08**	-0.07	-0.01	-0.11**	-0.15***	-0.03
	(0.04)	(0.06)	(0.03)	(0.05)	(0.04)	(0.08)
r2	0.97	0.95	0.97	0.91	0.88	0.92
N	16,594	15,497	16,427	16,479	15,787	16,264
C: County Uninsured Rates						
	Below Median Uninsured Rate			Above Median Uninsured Rate		
Treat*Post	-0.09**	-0.08	-0.03	-0.05	-0.13**	0.09
	(0.04)	(0.05)	(0.04)	(0.05)	(0.05)	(0.09)
r2	0.97	0.94	0.96	0.89	0.89	0.92
N	16,741	15,842	16,663	16,332	15,442	16,028

Note: Panel A shows OLS models. Panels B and C show OLS models weighted by the county population of children under six years of age. White robust standard errors are clustered at the county level. NCANDS administrative records of alleged child maltreatment reports 2010 – 2013 collapsed to the county level (N = 4,755,579). Covariates are included in all models (county-level unemployment rate, county-level poverty rate, child race, child age, family structure). State and month fixed effects are included as well. Standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1.

Table 1-5: DD estimates stratified by child race/ethnicity

	(1)	(2)	(3)	(4)	(5)
	All	Physical	Neglect	Rate of Subst. Reports	Foster care rate
A: White					
Treat*Post	0.00	-0.14**	0.01	-0.01	-0.02**
	(0.02)	(0.06)	(0.03)	(0.01)	(0.01)

r2	0.94	0.88	0.93	0.56	0.96
N	32,904	29,370	32,394	32,904	28,322
B: Black					
Treat*Post	-0.03	-0.15**	-0.02	0.01	-0.02**
	(0.03)	(0.06)	(0.03)	(0.02)	(0.01)
r2	0.96	0.91	0.95	0.42	0.94
N	29,527	20,353	27,678	29,527	23,001
C: Hispanic					
Treat*Post	-0.02	-0.07**	-0.01	0.00	-0.02**
	(0.02)	(0.03)	(0.03)	(0.01)	(0.01)
r2	0.97	0.93	0.96	0.37	0.93
N	27,828	17,245	24,745	27,828	20,316
D: Other					
Treat*Post	-0.03	-0.19***	0.02	0.02**	-0.02**
	(0.04)	(0.07)	(0.05)	(0.01)	(0.01)
r2	0.92	0.82	0.90	0.29	0.93
N	23,701	12,466	20,341	23,701	17,260

Note: OLS models weighted by the county population of children under six years of age. White robust standard errors are clustered at the county level. NCANDS administrative records of alleged child maltreatment reports 2010 – 2013 collapsed to the county level (N = 4,755,579). Covariates are included in all models (county-level unemployment rate, county-level poverty rate, child age, family structure). All models include state fixed effects, month fixed effects, and county-linear time trends. Standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1.

Table 1-6: DD estimates stratified by child age

	(1)	(2)	(3)	(4)	(5)
	All	Physical	Neglect	Rate of Subst. Reports	Foster care rate
A: Infant (< 12m)					
Treat*Post	-0.02	-0.02	-0.01	-0.01	-0.02***
	(0.02)	(0.08)	(0.02)	(0.01)	(0.01)
r2	0.96	0.89	0.95	0.63	0.96
N	32,409	21,935	31,505	32,409	26,882
B: One year					
Treat*Post	0.00	-0.21***	0.05	0.00	-0.02***
	(0.02)	(0.08)	(0.04)	(0.01)	(0.01)
r2	0.96	0.87	0.94	0.54	0.96
N	32,276	21,412	31,241	32,276	25,310
C: Two years					

Treat*Post	-0.03*	-0.15***	-0.01	0.01	-0.02***
	(0.02)	(0.06)	(0.04)	(0.01)	(0.01)
r2	0.96	0.87	0.94	0.54	0.96
N	32,421	22,818	31,399	32,421	25,344
D: Three Years					
Treat*Post	-0.02	0.02	-0.01	0.00	-0.02***
	(0.03)	(0.06)	(0.03)	(0.01)	(0.01)
r2	0.96	0.88	0.94	0.54	0.96
N	32,524	23,820	31,406	32,524	25,318
E: Four years					
Treat*Post	-0.02	-0.15***	-0.02	0.01	-0.02***
	(0.02)	(0.05)	(0.04)	(0.01)	(0.01)
r2	0.96	0.89	0.94	0.53	0.95
N	32,582	24,522	31,379	32,582	25,316
F: Five years					
Treat*Post	-0.04**	-0.11**	0.00	0.00	-0.02**
	(0.02)	(0.05)	(0.03)	(0.01)	(0.01)
r2	0.95	0.89	0.93	0.53	0.96
N	32,576	25,316	31,364	32,576	25,183

Note: OLS models weighted by the county population of children under six years of age. White robust standard errors are clustered at the county level. NCANDS administrative records of alleged child maltreatment reports 2010 – 2013 collapsed to the county level (N = 4,755,579). Covariates are included in all models (county-level unemployment rate, county-level poverty rate, child race, child age, family structure). All models include state fixed effects, month fixed effects, and county-linear time trends. Standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1.

Table 1-7: DD estimates stratified by child gender

	(1)	(2)	(3)	(4)	(5)
	All	Physical	Neglect	Rate of Subst. Reports	Foster care rate
A: Female					
Treat*Post	-0.02	-0.14***	-0.01	0.00	-0.02***
	(0.02)	(0.03)	(0.03)	(0.01)	(0.01)
r2	0.97	0.92	0.96	0.71	0.97
N	32,919	28,807	32,380	32,919	28,238
B: Male					
Treat*Post	-0.03*	-0.06*	-0.01	0.00	-0.02***

	(0.02)	(0.04)	(0.03)	(0.01)	(0.01)
r2	0.97	0.93	0.96	0.71	0.97
N	32,930	29,947	32,455	32,930	28,321

Note: OLS models weighted by the county population of children under six years of age. White robust standard errors are clustered at the county level. NCANDS administrative records of alleged child maltreatment reports 2010 – 2013 collapsed to the county level (N = 4,755,579). Covariates are included in all models (county-level unemployment rate, county-level poverty rate, child race, child age, family structure). All models include state fixed effects, month fixed effects, and county-linear time trends. Standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1.

Table 1-8: DD estimates stratified by financial hardship

	(1)	(2)	(3)	(4)	(5)
	All	Physical	Neglect	Rate of Subst. Reports	Foster care rate
A: No Public Assistance					
Treat*Post	0.01	-0.18 **	-0.06	0.00	-0.01
	(0.08)	(0.07)	(0.08)	(0.03)	(0.01)
r2	0.95	0.95	0.95	0.61	0.94
N	17,227	14,027	16,311	17,227	14,540
B: Public Assistance					
Treat*Post	-0.25***	-0.28**	-0.2**	0.02	0.00
	(0.08)	(0.13)	(0.09)	(0.02)	(0.01)
r2	0.94	0.91	0.93	0.59	0.92
N	14,730	10,065	13,810	14,730	13,539

Note: OLS models weighted by the county population of children under six years of age. White robust standard errors are clustered at the county level. NCANDS administrative records of alleged child maltreatment reports 2010 – 2013 collapsed to the county level (N = 4,755,579). Covariates are included in all models (county-level unemployment rate, county-level poverty rate, child race, child age, family structure). All models include state fixed effects, month fixed effects, and county-linear time trends. Standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1.

Table 1-9: DD estimates stratified by reporter type

	(1)	(2)	(3)	(4)	(5)
	All	Physical	Neglect	Rate of Subst. Reports	Foster care rate

A: Non-Medical Reporters					
Treat*Post	-0.03	-0.09***	-0.01	0.00	-0.02***
	(0.02)	(0.03)	(0.03)	(0.01)	(0.01)
r2	0.97	0.94	0.96	0.75	0.97
N	33,031	30,935	32,642	33,031	28,893
B: Medical Reporters					
Treat*Post	-0.03	-0.16*	-0.01	0.00	-0.01*
	(0.03)	(0.09)	(0.04)	(0.01)	(0.01)
r2	0.93	0.85	0.91	0.57	0.93
N	29,951	19,510	27,122	29,951	23,231

Note: OLS models weighted by the county population of children under six years of age. White robust standard errors are clustered at the county level. NCANDS administrative records of alleged child maltreatment reports 2010 – 2013 collapsed to the county level (N = 4,755,579). Covariates are included in all models (county-level unemployment rate, county-level poverty rate, child race, child age, family structure). All models include state fixed effects, month fixed effects, and county-linear time trends. Standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1.

Table 1-10: DD estimates, robustness checks with American Community Survey data

	(1)	(2)	(3)
	Living Out of Home	All Siblings and Children OOH	Lives with grandparent
A: Fixed effects only			
Treat*Post	-0.005*	-0.004*	-0.003**
	(0.00)	(0.00)	(0.00)
r2	0.02	0.03	0.08
B: Fixed effects and county-LTT			
Treat*Post	-0.003	-0.004*	-0.003*
	(0.00)	(0.00)	(0.00)
r2	0.02	0.02	0.04
N	793,734	793,734	793,734

Note: Panel A shows estimates of equation (1) using child-level OLS regressions with White robust standard errors clustered at the county-level. Panel B shows the same model as panel A, however with county-linear time trends included. OLS models weighted by the county population of children under six years of age. American Community Survey data 2010 – 2013. Covariates are included in all models (household head education, child race, child age). All models include state fixed effects and year fixed effects. Standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1.

Table 1-11: DD estimates, robustness checks

	(1)	(2)	(3)
	Log(all reports)	Physical Abuse	Neglect
A: State-level ACA Expansion			
Treat*Post	-0.09*	-0.12*	-0.08
	(0.03)	(0.05)	(0.04)
r2	0.96	0.93	0.96
N	51,783	48,857	51,165
B: California Counties Only			
Treat*Post	-0.04	-0.05*	-0.01
	(0.03)	(0.03)	(0.02)
r2	0.99	0.97	0.99
N	1,727	1,687	1,726

Note: Panel A shows estimates for the 2014 state-level expansion, with White robust standard errors clustered at the level of the state. Panel B shows the estimates with California counties only, omitting all other states, with White robust standard errors clustered at the county level. OLS models weighted by the county population of children under six years of age. NCANDS administrative records of alleged child maltreatment reports 2010 – 2013 collapsed to the county level (N = 4,755,579). Covariates are included in all models (county-level unemployment rate, county-level poverty rate, child race, child age, family structure). All models include state fixed effects, year fixed effects, and linear time trends. Standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1.

Table 1-12: Test of pre-trends in outcome variables

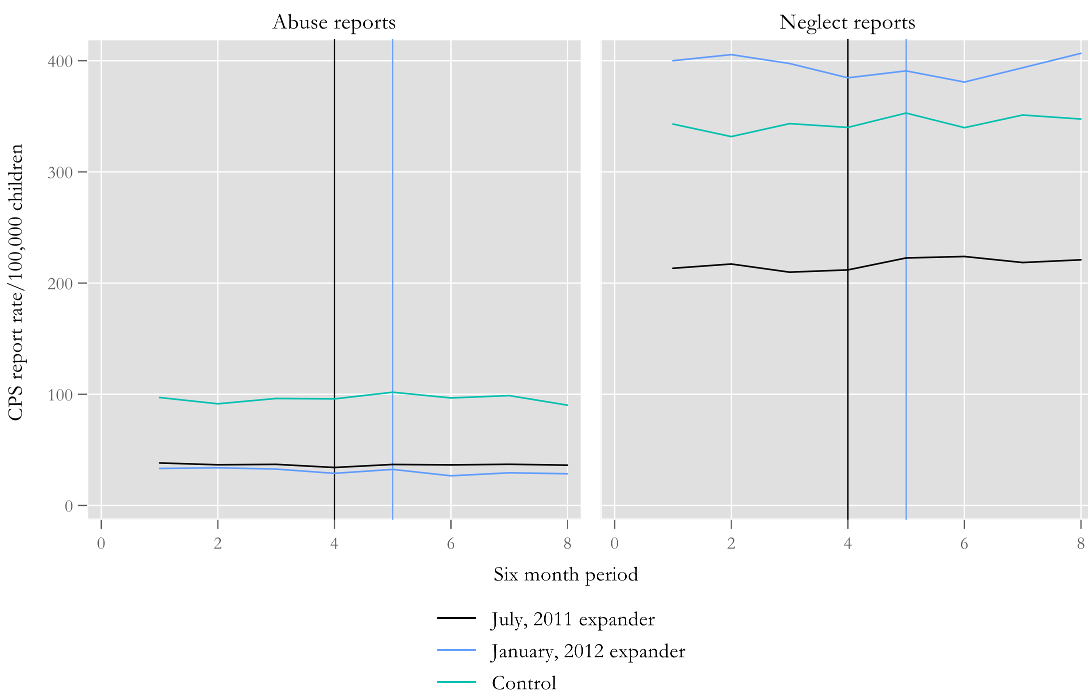
	(1)	(2)	(3)	(4)	(5)
	Log(all reports)	Physical Abuse	Neglect	Rate of Subst. Reports	Foster rate
Trend*Treat	0.00	0.00	0.00	0.00	0.00
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Trend	0.00	0.00	0.00	0.00	0.00
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Treat	0.20	-0.60***	0.86***	-0.12***	-0.91***
	(0.13)	(0.08)	(0.12)	(0.02)	(0.02)

Constant	6.83***	3.38***	2.90***	0.19***	0.19***
	(0.49)	(0.24)	(0.31)	(0.05)	(0.05)
N	12,301	11,686	12,176	12,301	10,365
r2	0.96	0.94	0.96	0.80	0.98

Note: OLS models weighted by the county population of children under six years of age. White robust standard errors are clustered at the county level. NCANDS administrative records of alleged child maltreatment reports in the pre-treatment period collapsed to the county level. Covariates are included in all models (county-level unemployment rate, county-level poverty rate, child race, child age, family structure). All models include state fixed effects and month fixed effects. Trend = monthly time trend, treat = indicator for treated county. Standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1.

Appendix

Figure 1-A1: Plot of CPS reports by maltreatment type, biannual data



Note: NCANDS administrative records of alleged child maltreatment reports 2010 – 2013 collapsed to the county – six month period – maltreatment type – month level. Graph shows the total CPS reports per 100,000 children for each county subtype (those that expanded in 7/2011, those that did so in 1/2012, and the control counties).

Table 1-A1: County expansion dates

County	County FIPS	Population (2017)	Compliance rate	Expansion date	County Medical Services Program (CMSP)	Omitted from data (< 1,000 reports)
Alameda	6001	1,663,190	95.6%	Jul-11		x
Alpine	6003	1,120	28.6%	Jan-12	x	x
Amador	6005	38,626	28.6%	Jan-12	x	x
Butte	6007	229,294	28.6%	Jan-12	x	
Calaveras	6009	45,670	28.6%	Jan-12	x	x
Colusa	6011	21,805	28.6%	Jan-12	x	x
Contra Costa	6013	1,147,439	35.7%	Jul-11		
Del Norte	6015	27,470	28.6%	Jan-12	x	x
El Dorado	6017	188,987	28.6%	Jan-12	x	
Fresno	6019	989,255	.	Jan-14		
Glenn	6021	28,094	28.6%	Jan-12	x	x
Humboldt	6023	136,754	28.6%	Jan-12	x	
Imperial	6025	182,830	28.6%	Jan-12	x	
Inyo	6027	18,026	28.6%	Jan-12	x	x
Kern	6029	893,119	11.0%	Jul-11		
Kings	6031	150,101	28.6%	Jan-12	x	
Lake	6033	64,246	28.6%	Jan-12	x	x
Lassen	6035	31,163	28.6%	Jan-12	x	x
Los Angeles	6037	10,163,507	33.5%	Jul-11		
Madera	6039	156,890	28.6%	Jan-12	x	
Marin	6041	260,955	28.6%	Jan-12	x	
Mariposa	6043	17,569	28.6%	Jan-12	x	x
Mendocino	6045	88,018	28.6%	Jan-12	x	
Merced	6047	272,673	.	Jan-14		
Modoc	6049	8,859	28.6%	Jan-12	x	x
Mono	6051	14,168	28.6%	Jan-12	x	x
Monterey	6053	437,907	.	Mar-13		
Napa	6055	140,973	28.6%	Jan-12	x	x
Nevada	6057	99,814	28.6%	Jan-12	x	
Orange	6059	3,190,400	29.4%	Jul-11		
Placer	6061	386,166	26.0%	Aug-12		
Plumas	6063	18,742	28.6%	Jan-12	x	x
Riverside	6065	2,423,266	16.9%	Jan-12		
Sacramento	6067	1,530,615	3.7%	Nov-12		
San Benito	6069	60,310	28.6%	Jan-12	x	x

San Bernardino	6071	2,157,404	20.4%	Jan-12		
San Diego	6073	3,337,685	24.3%	Jul-11		
San Francisco	6075	884,363	34.4%	Jul-11		
San Joaquin	6077	745,424	4.4%	Jun-12		
San Luis Obispo	6079	283,405	.	Jan-14		
San Mateo	6081	771,410	41.3%	Jul-11		
Santa Barbara	6083	448,150	.	Jan-14		
Santa Clara	6085	1,938,153	29.2%	Jul-11		
Santa Cruz	6087	275,897	14.3%	Jan-12		
Shasta	6089	179,921	28.6%	Jan-12	x	
Sierra	6091	2,999	28.6%	Jan-12	x	x
Siskiyou	6093	43,853	28.6%	Jan-12	x	x
Solano	6095	445,458	28.6%	Jan-12	x	
Sonoma	6097	504,217	28.6%	Jan-12	x	
Stanislaus	6099	547,899	.	Jan-14		
Sutter	6101	96,648	28.6%	Jan-12	x	x
Tehama	6103	63,926	28.6%	Jan-12	x	
Trinity	6105	12,709	28.6%	Jan-12	x	x
Tulare	6107	464,493	.	Mar-13		
Tuolumne	6109	54,248	28.6%	Jan-12	x	x
Ventura	6111	854,223	36.1%	Jul-11		
Yolo	6113	219,116	28.6%	Jan-12	x	
Yuba	6115	77,031	28.6%	Jan-12	x	

Table 1-A2: DD estimation, subsample

	(1)	(2)	(3)	(4)	(5)	(6)
	All reports	Physical Abuse	Neglect	Sexual Abuse	Psych. Abuse	Other
A: Log(reports), with county-LTT						
Treat*Post	-0.03	-0.11***	-0.01	-0.06	-0.02	-0.78***
	(0.02)	(0.03)	(0.03)	(0.04)	(0.07)	(0.09)
r2	0.97	0.94	0.97	0.88	0.95	0.95
N	33,073	31,284	32,691	26,123	10,583	8,643
B: Log(reports), with county-LTT and in CA, excluding CMSP counties						
Treat*Post	-0.02	-0.12***	0.00	-0.05	0.01	-0.78***
	(0.02)	(0.03)	(0.03)	(0.05)	(0.08)	(0.09)

r2	0.97	0.94	0.97	0.89	0.95	0.95
N	32,066	30,316	31,685	25,342	9,606	8,587

Note: OLS models weighted by the county population of children under six years of age. White robust standard errors are clustered at the county level. NCANDS administrative records of alleged child maltreatment reports 2010 – 2013 collapsed to the county level (N = 4,755,579). Covariates are included in all models (county-level unemployment rate, county-level poverty rate, child race, child age, family structure). LTT = linear time trends. State and month fixed effects are included as well. Standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1.

Table 1-A3: DD estimation, biannual data using three county subtypes

	(1)	(2)	(3)	(4)	(5)	(6)
	All	Abuse	Neglect	All	Abuse	Neglect
Treat * Post	-0.634*	-0.129	-0.006	-0.640*	-0.140*	-0.009
	(0.110)	(0.035)	(0.011)	(0.102)	(0.027)	(0.016)
Constant	-8.991	6.594	7.742*	-10.481	6.46	7.484*
	(2.607)	(1.611)	(0.859)	(3.790)	(1.582)	(0.959)
r2	0.978	0.999	0.999	0.979	0.999	0.999
N	192	144	144	192	144	144
County type FE	x	x	x	x	x	x
Biannual FE	x	x	x	x	x	x
Month FE	x	x	x	x	x	x
Controls	x	x	x	x	x	x
County LTT	x	x	x	x	x	x
County LTT2				x	x	x

Note: OLS models weighted by the county subtype population of children under six years of age for each county subtype (those that expanded in 7/2011, those that did so in 1/2012, and the control counties). Results are identical with and without clustering on county subtype. NCANDS administrative records of alleged child maltreatment reports 2010 – 2013 collapsed to the county subtype – six month period – maltreatment type – month level. Covariates are included in all models (county-level unemployment rate, county-level poverty rate, child race, child age, family structure). LTT = linear time trends. LTT2 = squared linear time trends. Standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1.

VII. Paper 2: ECEC Programs in the United States: Does Access Improve Child Safety?

Abstract

A growing body of literature contends that Early Childhood Education and Care (ECEC) programs should generally prioritize funding on the intensive margin on the basis that the adulthood human capital returns to quality are greater. Yet, little is known about the lower-bound benefits to ECEC programs that have the potential to counterbalance the push for quality. In other words, there may be an unmeasured value to expanding access over increasing quality for fewer children, unveiling a potential set of funding priorities for states looking to expand ECEC programming. Child maltreatment offers important insight into this question as it is a non-trivial metric of well-being that derives in part from time spent with children. Indeed, respite from children can not only increase labor market participation and education, it can plausibly increase the quality – and safety – of care as well. In this paper, I asses the role of ECEC program access and generosity on child maltreatment as measured by Child Protective Services (CPS) reports. Using county-year administrative data covering the census of CPS reports in the United States, I estimate the role of CCDF child care subsidies, Head Start/ Early Head Start, State pre-kindergarten (SPK) and Universal pre-kindergarten (UPK) programs on CPS reports using OLS and multilevel models. In addition, I exploit exogenous variation in SPK enrollment surges to compare the CPS reports across states using a difference-in-difference-in-difference (DDD) framework. My results suggest that the largest welfare gains in program access may occur among the youngest children (under age three years), the group for whom program access is most constrained. However, findings from more robust, state-level models provide weak overall evidence of a lower-bound benefit.

Introduction

In the past 10 years, federal funding for Early Childhood Education and Care (ECEC) programs has increased by 62 percent.[7] In the past two years alone, discretionary support for the Child Care and Development Block Grant has nearly doubled, with similar gains for Head Start and Early Head Start (HS/EHS) and state pre-kindergarten (SPK/UPK) programs, signifying a shift toward early childhood investments that dually benefit families. However, ECEC program access, funding, and the overall cost of care varies widely across states, resulting in disparate access for the most vulnerable children.

Though prior work links high quality, holistic ECEC programs to parental educational and employment and children's long-term health and human capital, a central question is whether policymakers should favor universal ECEC programs that are open to children of all socioeconomic backgrounds, or whether higher-quality programs targeted to a smaller population of more disadvantaged children are more beneficial. As low-income families rely on informal kinship and under-regulated care in the absence of a suitable substitute, ECEC programs have the potential to convey additional benefits in terms of respite from de facto care. Though extant literature links Head Start to a reduction in Child Protective Services (CPS) reports relative to children who remained at home, all other caregiving scenarios conveyed plausibly similar benefits (Zhai, Waldfogel, & Brooks-gunn, 2013). By extension, ECEC

[7] https://www.ffyf.org/federal-funding-for-early-childhood-programs-a-decade-of-bipartisan-progress/

programs might plausibly yield similar effects on child maltreatment rates. Yet, little is known about the child safety benefits of ECEC generosity and access.

This paper provides a first look at the cumulative role of ECEC programs on child maltreatment rates in the US. I answer two key questions. My first question asks whether trends in ECEC program availability track CPS reports. Using fixed effects and multilevel models (MLM), I first examine the relationship between ECEC program enrollment rates and administrative CPS reports to determine whether enrollment or program availability offsets the need for CPS services. I then employ a series of difference-in-difference-in-difference (DDD) models, comparing the CPS caseloads of states before and after a doubling in enrollment to those with stable enrollments. For my second question, I dig deeper into the relationship between SPK/UPK programs to understand the specific role of these programs in potentially offsetting CPS caseloads. Exploiting the cross-state variation in program availability type and eligibility date, I estimate event history models comparing children in states with SPK/UPK programs to those without leading up to and following the age-based eligibility cutoff dated eligibility cutoff date, making parallel comparisons across UPK/non-UPK states.

My analysis makes several contributions to prior literature. First, my preferred, state-level models provide little evidence of a lower-bound effect of improved access on CPS report rates, implying that policymakers should push for improved program quality for the most disadvantaged children. Second, I advance the literature seeking to leverage existing policies to reduce and prevent child maltreatment. This is also the first study to combine detailed features of SPK and UPK programs. This paper proceeds as follows. Following a short description of child maltreatment and the three major ECEC programs with which this study is concerned, I discus the datasets used in my analysis, followed by my empirical methodology and results. My discussion of potential mechanisms and conclusion follows.

Background: child maltreatment

Child maltreatment is a metric of impoverishment, stress, and household and relationship instability (Doyle & Aizer, 2018).

Child maltreatment reports have increased by 12.2 to 18.8 percent since 2013[8] with disparities concentrated in high poverty geographies with relatively thin labor markets and conservative social safety nets (U.S. Department of Health & Human Services Families et al., 2016), there is a strong, theoretical argument for prevention. Child maltreatment has been linked to negative consequences for young children throughout adulthood (see e.g. Berger & Waldfogel, 2011; Doyle & Aizer, 2018). Adults who were maltreated as children have compromised mental health and stability, employment, earnings, and other measures of human capital and well-being (Currie & Tekin, 2012; Currie & Widom, 2010b; Fry et al., 2018; J. P. Mersky & Topitzes, 2010; Perez & Widom, 1994). Though causal studies of this nature are rare, observational and cohort studies agree with the burgeoning human capital production literature that early childhood is the most promising window for intervention (Almond & Currie, 2011; Almond et al., 2017; Currie & Rossin-Slater, 2015; Heckman & Masterov, 2007). Young children in particular face higher risks of maltreatment, with children under six representing half of all victims in the US.

[8] 12.2 percent increase in screened in reports, 18.2 percent increase in those that are were screened out https://www.acf.hhs.gov/sites/default/files/cb/cm2017.pdf

Zeroing in on at-risk children is particularly difficult as young children spend less time in front of mandated reporters than do school-age children, and young families are relatively disconnected to public assistance (Pac, Nam, Waldfogel, & Wimer, 2017). As a result, US child maltreatment prevention policies target young, low-income families who are known to CPS agencies, while the swaths of children unknown to CPS – many who plausibly face much higher risks – remain unknown to officials, receiving post-crisis interventions rather than prevention. Alternative strategies aim to reduce the overall risk of child maltreatment by expanding access to and generosity of the social safety net. Numerous studies have linked increases in income via cash and credit benefits and minimum wages to reduced CPS caseloads, suggesting that stress and income are primary conduits for maltreatment (Berger et al., 2016; Cancian et al., 2013; Paxson & Waldfogel, 1999a, 2002, 2003b; Raissian & Bullinger, 2017; Wildeman & Fallesen, 2017). Stable employment appears integral to the child maltreatment equation as well, differentially so among men, especially in high-unemployment areas with a male-heavy workforce (Lindo, Schaller, & Hansen, 2018).

Policy Landscape

Though not all ECEC programs are endowed with the same set of aims, their overlapping populations and services result in a set of choices that are close suitable substitutes, especially among children age three and four years, who are eligible for all three programs.

Head Start/Early Head Start (HS/EHS) and the Child Care and Development Fund child care subsidy program (CCDF) are the two largest federally sponsored ECEC programs that provide care, preschool, and services for young children and their families. State pre-kindergarten (SPK) programs are state-run preschools for three- and four-year old children, operating using a blend of state, local, and federal funding. Unlike SPK programs that are generally means-tested or require some other proof of hardship for eligibility, universal pre-kindergarten (UPK) programs are a subset of SPK programs, opening seats to all families regardless of income and hardship.

Though CCDF is the largest funded program in terms of childcare services for children from birth to age 13, only 15 percent of eligible families receive subsidies. Bipartisan support for childcare has boosted funding for CCDF in recent years, most recently an additional $5.8 Billion for the CCDBG in 2018, in part to boost the stipend amounts such that the net cost to families is much lower and to increase the number of certified providers (Child Care Aware of America, 2018). For instance, a number of states have seen declines in childcare slots in recent years, ranging from five percent in Minnesota and Wisconsin to 9 percent in California. CCDF subsidies are remitted to a range of providers including informal kinship care (relatives, neighbors) and certified child care centers. Some states have relationships with CPS agencies such that CPS-involved families receive priority placements when a waitlist is in place. Prior work found that states with stalled enrollments on account of a waitlist were predictive of increased CPS rates, though the precise relationship between program enrollment and maltreatment has yet to be established (Klevens, Barnett, Florence, & Moore, 2015).

HS/EHS has a much broader reach than CCDF, serving up to 31 percent of eligible children age three to five years (HS, 7 percent for EHS).[9] In addition to providing high-quality care and education, HS/EHS conveys a wealth of health and parenting benefits to parents including

[9] https://www.nhsa.org/national-head-start-fact-sheets

parental training on developmentally appropriate discipline and care, health education and health services referrals, job training, with special services for children with disabilities, in foster care, and those with temporary or permanent homelessness. Unlike the other programs, HS/EHS have direct relationships with CPS agencies such that referrals are commonly made in both directions. For this reason, parsing out causality for these programs is more difficult, with more credible findings coming from individual-level randomized studies (see e.g. Zhai, Waldfogel, & Brooks-Gunn, 2013).

SPK and UPK programs served 1.5 Million children over the 2016-2017 school year, enrolling 33 percent of eligible children.[10] Though the number of states without SPK programs has fallen by half since 2002, states have experimented with changes in funding and program availability such that access is highly variable across states and time. Though gross funding has increased in most states, five have seen decreases in funding and the average per-child spend has declined to $5,008 on average. The most notable surge in funding came in 2014 through the Preschool Development Grants (PDG) afforded to 18 states (in 2016/2017) as part of President Obama's "Preschool for All" initiative. Though only 40 percent of PDF funding is dedicated to SPK/UPK programs, these funds are generally used to boost the number of seats, program quality, and for extended day programming.

[10] http://nieer.org/state-preschool-yearbooks/yearbook2017

Prior Literature

There is an extensive literature examining the role of ECEC programs in promoting rich childhood environments for low-income children. In general , the benefits of ECEC programs for young children are fairly well-established; there is ample evidence that high-quality, accessible programs enrich the lives of young children, impacting their short-term cognitive and non-cognitive ability and their human capital production through adulthood (Currie & Rossin-Slater, 2015; Gormley, Gayer, Phillips, & Dawson, 2005; Love et al., 2005; Magnuson, Meyers, Ruhm, & Waldfogel, 2004). However, these positive findings are not universal across all ECEC programs and are mixed for child care subsidies in particular; historically low levels of support and strict subsidy maintenance policies have been attributed to poorer child-level outcomes due to subsidy instability and lower overall quality of care (see e.g. Henly et al. 2017; Herbst and Tekin 2016; Forry, Daneri, and Howarth 2013).

ECEC programs convey benefits to parents as well. A smaller subset of studies focus on the role of ECEC programs on maternal labor force participation and human capital production. Although the results are somewhat mixed, CCDF child care subsidies and State Pre-K programs have been linked to improved maternal employment (Bainbridge, Meyers, & Waldfogel, 2003; Bettendorf, Jongen, & Muller, 2015; Blau & Tekin, 2007; Geyer, Haan, & Wrohlich, 2014; Givord & Marbot, 2014; Ha & Miller, 2015; Nollenberger & Rodríguez-Planas, 2013), however the effects of Head Start on employment are less conclusive (Sabol & Chase-Lansdale, 2015). Head Start and child care subsidies appear to spur additional education among receiving parents (Herbst & Tekin, 2011; Sabol & Chase-Lansdale, 2015). In addition to improved labor market participation, more generous ECEC programs might increase disposable income and reduce financial and parenting stressors, and effectively decrease the hours that a child is exposed to a potentially unsafe situation. For instance, adequate supervision has been linked to a reduction in injuries and child fatalities (Damashek & Corlis, 2017; Damashek, Nelson, & Bonner, 2013). Head Start and child care subsidies have been linked with improved maternal mental health and gentle parenting practices (Herbst and Tekin 2014; Love et al. 2005; Zhai, Waldfogel, and Brooks-Gunn 2013), although positive effects were not a universal finding (Ansari, Purtell, & Gershoff, 2016). A reduction in child maltreatment would naturally flow from this set of benefits.

A handful of causal studies found reduced maltreatment rates (by up to 50 percent) and out of home placement (foster care) among children enrolled in HS/EHS (Green et al., 2014; Klein, Fries, & Emmons, 2017; Zhai et al., 2013), and other public preschool programs, such as Chicago's Child-Parent Centers (J. Mersky, Topitzes, & Reynolds, 2011; Reynolds & Robertson, 2003). Observational studies found similar effects, however the results were mixed and generally unable to explore specific mechanisms. The majority of the studies exploring the maltreatment consequences of ECEC program involvement suffer from low power to detect CPS involvement. Additionally, few studies attempt to examine the mechanisms that contribute to a reduction in maltreatment, hampering the tuning and expanding of policies to further benefit vulnerable children. Furthermore, few of these studies detail the care received by children in the control group. Defining the forms of care being given to children in the control group is important, as many children are not receiving the counterfactual of 'no care' and instead, are faced with a large number of close, suitable substitutes (Kline & Walters, 2016). Within the taxonomy of 'other preschool' programs, there exists a wide variation in quality – even at the same price point – so if 'other preschool' is analyzed as a single homogeneous treatment, then implicitly, there are no assumed differences between a $40,000 private school and a state Pre-K program. Finally, none of those surveyed explored the maltreatment reducing effects of state Pre-K programs and CCDF child care subsidies.

Data

I combine several unique data sources to assess the role of ECEC programs in mitigating child maltreatment. I will discuss each in turn.

Data on child maltreatment comes from the National Child Abuse and Neglect Data System (NCANDS) Child File, a federally sponsored administrative data from 2000 to 2017 capturing the census of CPS reports for all states who voluntarily submitted data in this period, ranging from 20 states in 2000, 45 states in 2003, to all states in 2017. Each observation represents a report for a single child, such that children can appear multiple times in the data if they receive more than one CPS report within or across years. County identifiers are supplied in the restricted data for all cases with more than 1,000 reports per year, leaving a fraction of cases omitted from county-level analysis. As data are voluntarily suppled from state-level reporting systems with varying internal collection requirements, the availability of a number of variables is inconsistent across all states and years. For this reason, I restrict my analysis to the key measures of maltreatment (overall CPS report rate and that by major maltreatment type, substantiation rate, and removal / foster care rate), along with key demographic indicators (child race/ethnicity, age, sex, family structure) and mandated reporter type. Child maltreatment is measured by the natural log of the number of reports per county-year, though I include the report rate per 100,000 children. The residuals using the latter measure fail to follow a normal distribution, so include these in my primary estimates only for comparability to prior literature. To ease interpretation, I characterize measures of ECEC program availability similarly, as the log of enrollment and the rate of children served per 100,000 children in the county.

My second data set covers SPK/UPK enrollment, funding, and program characteristics. I scraped these data from NIEER State of Preschool Yearbooks from 2003 – 2017. NIEER conducts an annual survey of SPK/UPK programs to quantify the number and percentage of children served by the 60 programs in 43 states (Idaho, Montana, New Hampshire, North Dakota, South Dakota, Utah and Wyoming do not currently have SPK/UPK programs), program features, and quality metrics for benchmarking gains (or losses). These data are merged to the NCANDS data at the state-year level.

My third data set captures HS/EHS enrollment by child age at the state-year level from Head Start's PIR administrative reporting data and Kids Count Data Center, and my fourth captures CCDF subsidy use and family, provider, and reimbursement statistics at the state-year level from AHS administrative CCDF reports. Both of these data are merged to NCANDS at the state-year level.

I use SEER for US population data by age at the county-year level for population levels, and draw state-year level controls from the University of Kentucky Center for Poverty Research (UKCPR), including the net effective minimum wage, state per capita income, unemployment rate, and benefit usage and generosity statistics. Finally, my seventh data come from the CCDF policy database, which captures state-level variation in CCDF policies - I extract state-year level information on policies relating to stipend availability and availability.

For my first question, I aggregate these data to a panel of county-year level observations, merging all extramural data sets using the state-year variables. As SPK/UPK programs operate during the school year, I merge these variables using the school year calendar (August – June) rather than the calendar year. For my second question, I use the same data aggregated to the county-month level, to estimate changes in CPS report rates in the months leading up to and following an SPK/UPK introduction or expansion.

As the rate of substantiation and rate of removal from foster care reflect county and state-level norms, population characteristics, differing definitions in maltreatment, and varying degrees of prevention program generosity, these measures are typically considered laden with measurement error relative to the 'true' rate of child maltreatment. The CPS report rate is conceived as the least biased measure of child safety, though use of this measure necessitates a number of caveats as well. Though it would seem straightforward to interpret a decline in CPS reports as a decline in the 'true' child maltreatment rate, the opposite could be true as well. If a policy or intervention results in an increase in CPS reports, it could be that there is no change in the relative safety of the children, but instead a shift in the willingness of mandated or non-mandated reporters to generate a report. Thankfully, this is a fully testable assertion in this study, where the nature of the caller is known.

Empirical methodology

I exploit variation in the timing and generosity of ECEC programs across states to estimate the effect on of program enrollment on CPS reports. For my first question, I employ county-year and state-year panel models using fixed effects with robust standard errors clustered at the state level. Though I present results for both estimations, those at the state-year level are subject to less bias and are therefore preferred. For the county-level models, I estimate the following:

(1) $y_{ct} = \alpha + \beta C_c + \rho T_t + \gamma X_{ct} + \delta ECEC_{st} + \varepsilon_{ct}$

Where y is the outcome for children in county c in year t, C is a county fixed effect, T is a year fixed effect, X is vector of macroeconomic controls, $ECEC$ captures the program enrollment.[11] ε_{ct} is a random error term, capturing the unobserved factors and characteristics that predict CPS report rates such as mandatory reporting policies, child maltreatment fatalities, and relative ease of reporting. All models are estimated using OLS weighted by the county population of children younger than six years.[12] In addition to the inclusion of the common macroeconomic controls (maternal unemployment rates, per capita income, and effective minimum wage), I include the state EITC rates, along with demographic composition including race/ethnicity, age, and family structure. I include county and year fixed effects to control for secular differences in CPS reporting rates across counties and over time.

To the extent that the error term is uncorrelated with ECEC, the unbiased estimated effect of ECEC programs is captured by δ. However, there are several reasons why unbiasedness may be an implausible. For instance, more child-centric states may be prone to SPK/UPK programs and to funding child maltreatment prevention programs. In this case, δ captures the effect of state generosity as well as that of ECEC programs. I deal with this in two ways; first, I test the robustness of my results to the inclusion of county-specific linear time trends to account for unobserved factors that vary within counties over time. Under the condition that unobserved factors across counties exhibit a monotonic trend, these models should fully account for any

[11] As the income eligibility standards for two programs differ by state (State pre-K and CCDF subsidies), this choice captures eligibility differences across states as well, so an alternative specification includes the fraction enrolled out of the state population of children who are only age-eligible as a broader measure of generosity, including income eligibility thresholds as a separate variable.
[12] Age-stratified models use the age-appropriate weights

remaining bias. The key identifying assumption is that treatment (enrollment) rates are uniform across counties. Though not untenable, it is more likely that counties with more concentrated poverty will have higher program enrollment rates than their more advantaged peer counties, implying that my estimates are relatively conservative.

Though this approach addresses concerns with omitted variables, reverse causality might threaten the validity of my results. If ECEC program expansions increase the number of seats to families who would otherwise have kept their children at home, childcare providers might be more likely to report at-risk children to CPS. Childcare providers are mandated reporters in 48 states and DC, meaning that failure to report could result in criminal prosecution. If reverse causality is indeed driving ECEC effects, the ensuing bias would inflate my estimated effects, biasing my hypothesized negative effects away from zero. However, the degree of bias is unlikely in a fixed effect framework, where the remaining variation in CPS report rates, after the inclusion of controls and fixed effects, is likely random. Nevertheless, to account for this potential threat, I run the same models stratified by the type of reporter notated on the CPS report, either childcare / educational or not, ruling out or rendering support to the reverse causality problem.

While the fixed effect framework is commonplace in the micro econometric toolkit, the underlying empirical assumption in equations (1) and (2) is that the residuals on the state-level are not correlated with county-level residuals in any meaningful way. Though this assumption may be tenable in some applications, true dependence between and within geographies over time implies that any non-random correlation would bias fixed effects estimates (omitted variable bias). The Hausman test can be informative in answering the fixed vs. random effect question, though the test is uninformative in deciding between mixed effects and fixed or random effects.

That the test fails to reject the null (in favor of fixed effects) does not mean that intuition is ignorable. If we truly believe that the between effect of states on county-level CPS reports is negligible, then equation (1) is feasible. Alternatively, if we believe that the state-level effect is not static, then the relationship between and within counties and states must be incorporated into the estimation. Indeed, likelihood ratio tests confirms that random slopes should be included, relative to the random intercept-only model. For this reason, I employ multilevel models (mixed effects models, referred to henceforth as MLM) in addition to the conventional fixed effects framework for my primary models. This framework incorporates two random terms - a random intercept and slope at the state level, allowing heterogeneity in the within-effect across states. These effects follow a Normal distribution. This formulation more fully accounts for differences across counties and states over time, information that is effectively 'thrown away' in the fixed effect framework. I include state-level means of the predictors of interest to account for group- and predictor- level correlation (Bafumi & Gelman, 2006). These models are estimated using Maximum Likelihood Estimation.

For my second question, I ask whether SPK/UPK programs in particular offset child maltreatment as measured by CPS reports. To do so, I use two parallel two approaches. First, I employ a series of difference-in-difference-difference (DDD) estimations that capture the effect of large, state-specific shocks for SPK/ UPK programs that boosted enrollment by over 50 percent, comparing eligible children to those who are ineligible (on the basis of age in months). I compare UPK programs separately from SPK programs. DDD models are characterized by equation (2) below, where Post is coded as "1" for states that experienced a large discontinuous shock and "0" otherwise, with eligibility denoting four year old children apart from those who are ineligible (younger than three and five years, omitting three year old children entirely):

$$(2)\ y_{ct} = \alpha + \beta C_c + \rho T_t + \gamma X_{ct} + \pi Elig_{ct} + \theta POST_{st} * Elig_{ct} + \varepsilon_{ct}$$

I estimate equation (2) as a separate OLS model for each program using the same weighting and clustering strategies defined above. Unlike standard difference-in-difference models, DDD models allow for a weaker set of assumptions. In particular, a test of common trends in the outcome variables in the DD context reveals that the parallel trend assumption is indeed, untenable (see appendix figure 2-A1). DDD models compare the outcomes across program availability and eligibility, formulating an Intent-to-Treat (ITT) effect that is interpreted as relative.

Second, I ask whether children's relative likelihood of maltreatment declines upon gaining eligibility for SPK and UPK programs. This serves as more precise test for causality, either ruling or confirming that program effects occur only among age-eligible children. Using county-month data where I observe the child's age in months, I employ an event history model extrapolating eligibility for SPK/UPK programs in a given month based on the match between their age in months relative to the program's cutoff date. I construct a series of leads and lags before and after the cutoff date for a 6 month bandwidth to ensure that that the program does not coincide with other programmatic changes throughout the school year. I estimate the following model using both OLS and MLM as characterized in equation (3) below:

$$(3)\ y_{ct} = \alpha + \beta C_c + \rho T_t + \gamma X_{ct} + \textstyle\sum_{j \in J} \theta_j POST_{st}^{j} + \varepsilon_{ct}$$

Where $POST_{st}^{j}$ is a series of dummy variables equal to 1 for the states in which UPK was in place for *j* periods, J ={-6, -5, -4, -3, -2, -1, 0, 1, 2, 3, 4, 5, 6} with the month of the eligibility start as the omitted category (0). I estimate these models for all SPK/UPK programs combined, then separately for UPK programs only. I include county-specific linear time trends in the results shown as well.

Results

My sample follows the expected distributions of maltreatment type, child age, and race/ethnicity, as seen in the summary statistics reported in table 2-1. The major maltreatment type is neglect (61.2 percent), with physical abuse reaching only 17 percent of reports. The majority of children are under the age of 12 months (20.4 percent).

In the following section, I present the results from county-level models. Following, I present my preferred set of more robust, state-level models.

County-level results are presented in table 2-2. Each coefficient represents an individual regression, with OLS regressions in panel A, and multilevel models in panel B. As the residuals on the CPS report rate/100,000 are not normally distributed (columns 4 through 6), I rely on the log of CPS reports for the remainder of the paper. This sample includes all children under age six whose CPS reports are collapsed to the county-year level, with the demographic and macro controls discussed above. Across the two outcomes, the direction and magnitude of effects is not consistent, though there are some patterns of note. In the OLS models, CCDF enrollment has a mostly negative effect, meaning that an increase in CCDF enrollment is associated with a decline in CPS reports up to a magnitude of 32 percent for physical abuse (column 2, panel A). These findings generally track the multilevel models (panel B), though the MLM coefficients are more precisely estimated. The very large magnitude in the effects of CCDF enrollment is concerning, motivating several robustness checks in the following section using state-aggregated data.

If ECEC program enrollment is indeed driving these effects, then the programs should be more or less effective for subgroups of the population. I focus on two subgroups in particular, for whom the effects of ECEC program availability may be more salient. First, I estimate the same models on a subset of the sample that includes only three and four-year old children as these children are generally eligible for all three programs. These findings, presented in table 2-3, follow the same general pattern, though magnitudes are relatively smaller and are more consistently negative for all programs with the exception of SPK/UPK programs (row 1, table 2-3, panel A). SPK is associated with a 2 percent increase in all reports (panel B, column 1), but a 3 percent decline in abuse reports and a 12 percent increase in neglect reports (columns 2 and 3). Conversely, CCDF enrollment is uniformly associated with a decline in all reports and the two subtypes – up to 37 percent – while HS/EHS is associated with a decline only in all reports (column 1). Note that the magnitudes for both estimations (tables 2-2 and 2-3) are somewhat smaller when county-specific linear time trends are included (appendix table 2-A2).

Second, I stratify by a proxy for socioeconomic status using an indicator for whether the child's caregiver at the time of the CPS report answered "yes" to the question "are you currently on any form of public assistance?" The group without public assistance, shown in columns 1 – 3 of table 2-4, show generally smaller coefficients that follow the same magnitudes with the exception of total program enrollment. The group with public assistance appear to be most affected by CCDF subsidy availability, though the magnitudes are smaller for abuse (column 5 vs 3) in the multilevel models (panel B) and equal for the fixed effects models (panel A, columns 5 and 3). The SPK coefficients are positive for both specifications and significant for all and neglect reports in the MLM context (panel B), implying that SPK program availability may indeed boost reports. This could be due to what is often referred to the 'visibility effect', where an increase in reporting is due to the reporting behavior of SPK teachers and administrators rather than an actual change in maltreatment rates. As I discuss in the following section, I find no evidence of differential reporting by child care/educational reports and those who are not, suggesting then that these effects may be anomalous, or simply biased by measurement error. Individual level data with actual ECEC program enrollment would better inform this question.

In table 2-5, I focus on CCDF and HS/EHS availability for children younger than three. As caregiving slots are the most competitive – and the most expensive – for children in this age group, enrollment increases might directly reduce the caregiving burden for young children exclusively. These results suggest that CCDF subsidies appear to be effective in reducing CPS report rates among all children younger than three years, especially reports of neglect (column 3). For children younger than 12 months, both programs appear to reduce abuse reports, though again, the magnitudes are relatively large. Though these results are not causal, they suggest that future research should examine this relationship more closely using experimental or quasi-experimental methods.

The results presented up to this point are correlational. Though I attempt to address reverse causality in the following section, other threats to causality – namely, omitted variable bias – deem these effects observational in nature. Difference-in-difference models would be a natural approach, though are biased in this case on account of differing pre-trends in CPS reports (see figure 2-A1). To estimate a more plausibly causal effect of program enrollment on CPS reports, I estimate DDD models to compare the CPS report rates across eligible and ineligible children in states with a surge in SPK/UPK program enrollment,[13] compared to those without. I present these findings in table 2-6 with fixed effect (OLS) models shown in panel A, and MLM shown in

[13] There were no major enrollment surges in the other programs, so this section focuses exclusively on SPK/UPK programs

panel B. I code four year old children as eligible, omit three year old children whose eligibility varies, and include all others (younger than age six) as ineligible. I find that a jump in enrollment of at least 50 percent is associated with a 2 to 4 percent decline in all reports (column 1, panels A and B). Though the main effects are mixed, that the interaction is mostly null and insignificant in the MLM context (panel B) suggests that these programs fail to credibly offset CPS involvement. I next examine two features of SPK programs to test whether specific policy levers are important in offsetting CPS report rates.

SPK programs generally enroll children based on expressed and demonstrated need, whereas UPK programs are indeed universal, meaning that there are no requirements for enrollment other than age-based eligibility. UPK programs offer similar programming, and like SPK programs, often blend funding from multiple sources to better serve children's particular needs. If SPK programs in general fail to affect CPS report rates, it could be that this is because they're catering to a population of children for whom the risk of maltreatment is already observed and established. Alternatively, UPK programs are open to all – regardless of CPS involvement or poverty status – so may reasonably offset CPS report rates among children with unobserved risk. I therefore estimate equation (2) using enrollment jumps in UPK programs exclusively. These results, shown in table 2-7, suggest that jumps in UPK enrollment are positively associated with abuse reports (column 2). Ecological fallacy plagues these estimations, as I am unable to observe whether children use SPK or UPK. Therefore, I now examine this relationship more closely in an event history model.

One key eligibility requirement for SPK/UPK programs is that children meet a minimum age requirement. I collapse county-year level data to children's age-in-months to test whether gaining eligibility affects CPS reporting rates across maltreatment type. This event history approach focuses on children living in states with any SPK program (figure 2-1) and any UPK program (figure 2-2). Comparing across children in age-eligible and ineligible groups, there are no consistent differences in maltreatment rates in the post-eligibility period. MLM models using the same approach yield the same patterns estimated with slightly greater precision (that for SPK programs is shown in figure 2-A2 in the appendix). These offer the strongest evidence yet that SPK and UPK programs fail to offer a lower-bound benefit to four-year old children. These results are not surprising as typical programs run for 2-4 hours per day.

Several SPK/UPK programs contract with local, CCDF subsidy, and HS/EHS providers to extend the school day beyond the half- or school-day schedule. I test the salience of this program feature using a DDD model. The results, shown in table 2-8, show small, insignificant effects, implying that SPK/UPK program generosity in terms of hours per day is ignorable to CPS rates.

Robustness Checks

Though a subset of these findings imply a negative correlation between program enrollments and CPS reports, there are some positive effects that cannot be ignored. In particular, SPK programs and HS/EHS appear to have a positive effect on neglect, suggestive of either a spike in reporting or maltreatment from these caregivers themselves. To test this, I estimate the same models on two subsets of children – those whose reports were generated from child care or education-based mandated reporters and all others. I regress the same models on both subgroups, as shown in table 2-9. I find that the patterns for both non-Child care reporters (columns 1 – 3) and child care reporters (columns 5 – 8) are similar with the exception of HS/EHS, which changes direction from negative to positive (columns 3 and 6, both panels). This suggests that the effects I detect are not an artifact of reverse causality (the coefficients would have all been positive if this were the case).

One problem with using CPS report rates as a measure is that it imperfectly captures actual child maltreatment. Measures of substantiation and removal from the home are biased as outcomes, as there are vast differences in defining (and punishing) maltreatment across states. Still, differential effects in these outcomes could point to differences in maltreating behavior, as opposed to reporting. In table 2-A1 in the appendix, I show the results from estimating equation 1 on the rate of removals into foster care (columns 1 and 3) and the rate of substantiated reports for each of three ECEC programs. I detect decreases in the removal rate for nearly all outcomes, with small insignificant increases in substantiation for all except for CCDF (columns 2 and 4), for whom the effects are negative and marginally significant. These findings imply that if CCDF is indeed responsible for the negative effects I detect earlier, it could be because access to this program alters parental behavior. In other words, in the absence of CCDF, parents might be more likely to maltreat their children.

There are a number of differences in ECEC program implementation across states, many of which are exogenously implemented, allowing for a causal test of potential mechanisms. If respite is indeed, the primary pathway through which ECEC program access improves child maltreatment rates, policy changes affecting the underlying mechanisms such as stress and income should have indistinguishable effects. I expect ECEC programs to reduce maltreatment reports through four potential pathways; (1) increase in disposable income and a reduction in financial hardship (2) increased parental labor market participation (3) a relatively safe caregiving scenario with increased supervision by outside observers and (4) a reduction in parental stress due to the child being partially out of the home and due to being exposed to other strategies for discipline.

The results from SPK-specific estimations above suggest either that these programs are too small in scale to reasonably affect children and families at risk of CPS involvement, or that parent's time, income, and stress – the pathways through which SPK programs might reduce maltreatment – remains largely unaffected. Yet, there are two features of CCDF subsidy programs that may offer additional insight into these potential mechanisms.

Unlike SPK and HS/EHS, CCDF child care subsidies require caregivers to remit a copay to providers that varies by state which could also directly affect the amount of disposable income offset by the program. States with higher copayments can be compared to those with lower copayments at similar income eligibility thresholds, allowing for a direct test on the effect of an increase in disposable income on child abuse and neglect, presumably having the largest effect on neglect. The key assumption is that parents in states with higher copays receive the same amount of care, presumably using other sources of care to offset the gap at the same rate as parents with lower copay amounts. Similarly, states with lower income eligibility thresholds restrict program access to only the most impoverished, reducing overall access and increasing child care costs. Both of these program features are exogenously implemented and change from year-to-year, such that a regression of these policy features on CPS report rates may reveal income to be a potential mechanism. The results from estimating equation (1) using these policy features (table 2-A7 in the appendix) suggest that while income eligibility is likely not to affect CPS rates, states with lower copay amounts have .05 – 1.1 percent fewer abuse reports (column 5).

One way to reconcile the differences in results across specifications in my primary results is to aggregate the data to a higher-level, such that the need for county-level fixed effects, time trends, and the MLM are eliminated. Further, this approach addresses any concern about the assumption that county-level ECEC enrollment rates are uniform across counties. In tables 2-A4 – A6 in the appendix, I replicate my primary results using data collapsed to the state x age x race/ethnicity x reporter x maltreatment type. These results (shown exclusively with state and year fixed effects and state-specific linear time trends) yield a noticeably different pattern. For instance, table 2-A4 shows the results from estimating ECEC program enrollment on each of three samples – all children under age six (panel A), preschoolers (panel B) and children younger than 3 (panel C). SPK has a positive effect on all and neglect reports in all three samples (columns 1 and 3) which is consistent with earlier results. The effects of CCDF are negative for neglect reports, but insignificant, and positive for other outcomes. If the effect I detected earlier for young children were indeed 'true,' one would expect this effect to be perhaps smaller in magnitude, not larger and insignificant. That SPK affects younger, ineligible children also serves as a sort of falsification test, suggesting that either I am detecting spillover effects from older siblings, or that my earlier results should be interpreted with great caution. I also implement the DDD models (table 2-A5) to find fairly similar results, though the main effects differ in terms of direction and magnitude, and the interactions are generally much smaller and insignificant.

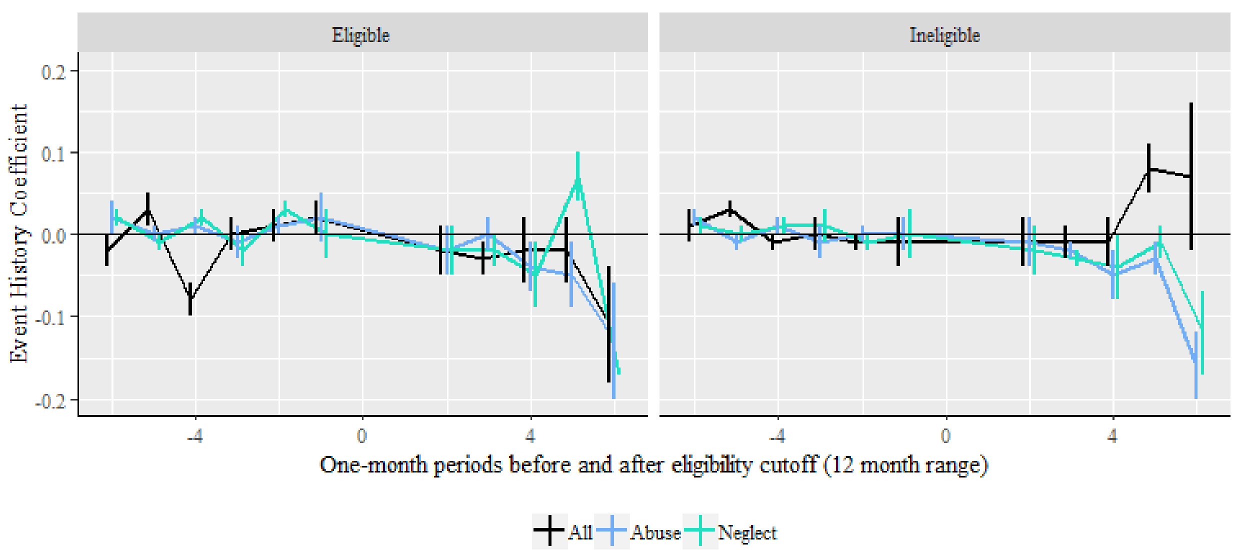
Conclusion

In this study, I estimate whether ECEC program enrollment reduces CPS caseloads, a measure of child maltreatment and safety. Though I find limited evidence that CCDF programs offset CPS reports for children younger than three, my preferred state-level results yield null results, suggesting that county-level findings should be interpreted with caution. For states looking to expand ECEC program offerings, the results from this study concur with prior evidence that state and local ECEC funding should indeed focus on program quality, as access alone appears to have little effect on CPS report rates. I find no evidence of reductions in reports from other programs, suggesting that either the programs themselves do not reach the population at risk of CPS reports, or that the program offerings alone are inadequate to affect child safety.

The primary limitation to this study is that ECEC uptake is unobservable at the individual level, so the results are interpreted as state-level effects of ECEC availability and generosity on child maltreatment reports. Second, I am unable to detect spillover that might occur within families and households. If care provided for one child enables parents to pay for care of other children is impossible to see in these data, as I am unable to link children within families.

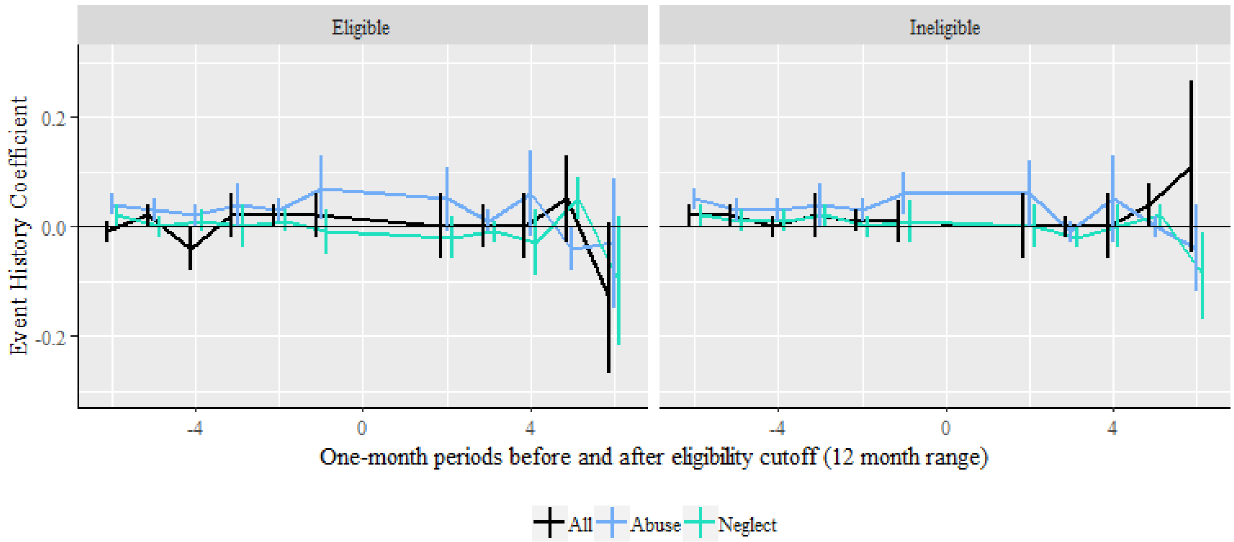
Figures

Figure 2-1: Event History (OLS) effect of SPK program availability



Note: This figure shows the coefficients and confidence intervals for event history regressions estimated using OLS on county-month aggregated data. Ineligible children – those younger than three years and age five years – are shown on the left, and eligible children – age four years – are shown on the right. Three year old children are omitted entirely due to their varying eligibility. All models include demographic and macro level controls.

Figure 2-2: Event History (OLS) effect of UPK program availability



Note: This figure shows the coefficients and confidence intervals for event history regressions estimated using OLS on county-month aggregated data. Ineligible children – those younger than three years and age five years – are shown on the left, and eligible children – age four years – are shown on the right. Three year old children are omitted entirely due to their varying eligibility. All models include demographic and macro level controls.

Tables

Table 2-1: Summary statistics

Variable	mean/freq.
Alleged Maltreatment type	
Physical Abuse	17.1%
Neglect	61.2%
Sexual Abuse	4.0%
Psychological	3.4%
Other	5.3%
No Maltreatment	9.1%
Child Race/ethnicity	
White	38.0%
Black	29.5%
Hispanic	24.9%
Other	7.6%
Child age	

Infant (< 12 months)	20.4%
1 year	15.4%
2 years	15.7%
3 years	15.8%
4 years	16.0%
5 years	16.7%
Observations	9,479

Table 2-2: Effect of ECEC program enrollment on maltreatment

	(1)	(2)	(3)	(4)	(5)	(6)
	Log(CPS reports)			CPS Report Rate / 100,000 children		
	All	Abuse	Neglect	All	Abuse	Neglect
Panel A: Fixed Effects						
SPK	0.02	0.07	0.12	32.07	-19.40	54.28*
	(0.02)	(0.08)	(0.07)	(28.79)	(11.54)	(26.66)
CCDF	-0.08	-0.32	-0.20	-50.50	-7.74	-220.85
	(0.09)	(0.29)	(0.19)	(136.54)	(33.44)	(143.08)
HS/EHS	-0.22	0.04	0.00	682.82	260.54	656.41
	(0.15)	(0.59)	(0.46)	(361.63)	(169.77)	(344.79)
Panel B: Multilevel models						
SPK	0.034*	0.02	0.12***	170.96***	11.58*	143.82***
	(0.02)	(0.02)	(0.02)	(13.38)	(4.81)	(12.91)
CCDF	0.045	-0.39***	-0.35***	138.01***	80.34***	29.49
	(0.04)	(0.04)	(0.04)	(30.78)	(7.20)	(21.79)
HS/EHS	-0.321*	0.27	0.32*	1173.88***	291.85***	928.27***
	(0.13)	(0.14)	(0.14)	(81.40)	(14.86)	(45.60)
N	9,479	9,403	9,448	9,479	9,403	9,448

Note: Panel A shows OLS regressions weighted by the county population of children under age six. Panel B shows multilevel models with random intercepts and slopes for counties nested in states. Enrollment for log outcomes, enrollment is measured in logs, and for rate outcomes, enrollment is similarly measured as rates per 100,000 children. County-year level aggregated data drawn from NCANDS administrative CPS reports from 2003 – 2017.

Table 2-3: Effect of ECEC program enrollment on maltreatment, 3- and 4- year old children

	(1)	(2)	(3)
	All	Abuse	Neglect
Panel A: Fixed Effects			
SPK	0.03	0.04	0.14
	(0.02)	(0.08)	(0.08)
CCDF	-0.05	-0.16	-0.23
	(0.08)	(0.22)	(0.23)
HS/EHS	-0.26	-0.18	-0.04
	(0.14)	(0.39)	(0.47)
Panel B: Multilevel models			
SPK	0.02*	-0.03**	0.12***
	(0.01)	(0.01)	(0.01)
CCDF	-0.1***	-0.18***	-0.37***
	(0.02)	(0.03)	(0.02)
HS/EHS	-0.31***	-0.02	0.11
	(0.07)	(0.10)	(0.09)
N	18,853	18,218	18,720

Note: Panel A shows OLS regressions weighted by the county population of children under age six. Panel B shows multilevel models with random intercepts and slopes for counties nested in states. Enrollment and CPS reports are measured in logs. County-year level aggregated data drawn from NCANDS administrative CPS reports from 2003 – 2017.

Table 2-4: Stratifications by public assistance

	(1)	(2)	(3)	(4)	(5)	(6)
	No PA			PA		
	All	Abuse	Neglect	All	Abuse	Neglect
Panel A: Fixed Effects						
SPK	0.16	0.01	0.08	0.09	0.01	0.13
	(0.15)	(0.09)	(0.10)	(0.14)	(0.10)	(0.12)
CCDF	0.12	-0.17	0.25	-0.59	-0.17	-0.44
	(0.57)	(0.42)	(0.56)	(0.57)	(0.35)	(0.54)
HS/EHS	0.28	-0.08	0.07	-1.58	1.38	-0.78
	(0.88)	(0.63)	(0.78)	(1.67)	(1.29)	(1.79)
N	4,900	4,488	4,783	4,979	4,174	4,735
Panel B: Multilevel models						
SPK	0.03	-0.03	0.04	0.21***	0.06	0.22***
	(0.03)	(0.03)	(0.03)	(0.04)	(0.03)	(0.03)
CCDF	-0.08	-0.45***	-0.01	-0.41***	-0.32***	-0.27**
	(0.09)	(0.07)	(0.08)	(0.09)	(0.08)	(0.09)

HS/EHS	0.31	0.37	0.02	-1.83***	0.67	-0.72
	(0.33)	(0.28)	(0.31)	(0.40)	(0.36)	(0.39)
N	4,900	4,488	4,783	4,979	4,174	4,735

Note: Panel A shows OLS regressions weighted by the county population of children under age six. Panel B shows multilevel models with random intercepts and slopes for counties nested in states. Enrollment and CPS reports are measured in logs. County-year level aggregated data drawn from NCANDS administrative CPS reports from 2003 – 2017. PA = caregiver reported receiving any type of public assistance.

Table 2-5: ECEC programs for young children

	(1)	(2)	(3)	(4)	(5)	(6)
	CCDF Child Care Subsidies			HS/EHS		
	All	Abuse	Neglect	All	Abuse	Neglect
Panel A: Fixed Effects						
< 12 months	-0.09	-0.40	-0.18	-0.30	0.07	0.00
	(0.12)	(0.39)	(0.22)	(0.19)	(0.83)	(0.46)
12 - 23 months	-0.08	-0.11	-0.26	-0.24	-0.08	0.02
	(0.08)	(0.22)	(0.23)	(0.12)	(0.46)	(0.49)
24 - 36 months	-0.10	-0.13	-0.26	-0.31	-0.15	0.01
	(0.08)	(0.24)	(0.23)	(0.18)	(0.44)	(0.51)
Panel B: Multilevel Models						
< 12 months	-0.17***	-0.51***	-0.32***	-0.30**	-0.46**	0.20
	(0.03)	(0.04)	(0.03)	(0.10)	(0.15)	(0.12)
12 - 23 months	-0.11***	-0.14***	-0.36***	-0.30**	-0.07	0.20
	(0.03)	(0.04)	(0.04)	(0.10)	(0.15)	(0.12)
24 - 36 months	-0.14***	-0.13**	-0.39***	-0.40***	-0.09	0.20
	(0.03)	(0.04)	(0.04)	(0.10)	(0.14)	(0.12)
N	9,421	9,106	9,382	9,421	9,106	9,382

Note: Panel A shows OLS regressions weighted by the county population of children under age six. Panel B shows multilevel models with random intercepts and slopes for counties nested in states. Enrollment and CPS reports are measured in logs. County-year level aggregated data drawn from NCANDS administrative CPS reports from 2003 – 2017. PA = caregiver reported receiving any type of public assistance.

Table 2-6: DDD effect of enrollment jump

	(1)	(2)	(3)
	All	Abuse	Neglect
Panel A: Fixed Effects			
Elig*jump	-0.04**	-0.07	0.00
	(0.01)	(0.05)	(0.02)
Jump	0.08	0.16	0.05
	(0.04)	(0.12)	(0.06)
Elig	-0.06***	0.15**	-0.17***
	(0.01)	(0.05)	(0.01)
Panel B: Multilevel Models			
Elig*jump	-0.02	0.01	-0.01
	(0.03)	(0.04)	(0.04)
Jump	0.02	0.11***	-0.07***
	(0.02)	(0.02)	(0.02)
Elig	-0.05***	0.14***	-0.14***
	(0.01)	(0.01)	(0.01)
N	47,126	45,635	46,895

Note: Panel A shows DDD regressions weighted by the county population of children under age six. Panel B shows multilevel models with random intercepts and slopes for counties nested in states. County-year level aggregated data drawn from NCANDS administrative CPS reports from 2003 – 2017. OLS models include county-specific linear time trends. Elig = child eligibility, 1 = age 4, 0 all other ages except age 3, who are omitted from this model. Jump = Binary indicator for whether an increase of enrollment of at least 50% occurred in the observed school year.

Table 2-7: DDD effect of UPK

	(1)	(2)	(3)
	All	Abuse	Neglect
Panel A: Fixed Effects			
Univ*Elig	-0.01	0.02	-0.01
	(0.01)	(0.08)	(0.02)
UPK	0.08	-0.03	-0.19
	(0.04)	(0.13)	(0.17)
Elig	-0.05***	0.15*	-0.17***
	(0.01)	(0.06)	(0.01)
Panel B: Multilevel Model			
Univ*Elig	0.01	0.07***	0.00
	(0.01)	(0.02)	(0.01)
UPK	0.05***	-0.1***	-0.25***

	(0.01)	(0.02)	(0.02)
Elig	-0.05***	0.11***	-0.14***
	(0.01)	(0.01)	(0.01)
N	47,126	45,635	46,895

Note: Panel A shows DDD regressions weighted by the county population of children under age six. Panel B shows multilevel models with random intercepts and slopes for counties nested in states. County-year level aggregated data drawn from NCANDS administrative CPS reports from 2003 – 2017. OLS models include county-specific linear time trends. Elig = child eligibility, 1 = age 4, 0 all other ages except age 3, who are omitted from this model. Univ = 1 for UPK introductions.

Table 2-8: DDD effect of extended day

	(1)	(2)	(3)
	All	Abuse	Neglect
Panel A: Fixed Effects			
Extday*Elig	-0.01	0.04	-0.02
	(0.01)	(0.05)	(0.01)
Extday	0.13**	0.27*	0.32*
	(0.05)	(0.13)	(0.12)
Elig	-0.05***	0.12**	-0.15***
	(0.01)	(0.04)	(0.01)
Panel B: Multilevel Model			
Extday*Elig	-0.01	0.01	-0.02
	(0.01)	(0.02)	(0.01)
Extday	0.09***	0.26***	0.3***
	(0.01)	(0.01)	(0.01)
Elig	-0.04***	0.13***	-0.13***
	(0.01)	(0.01)	(0.01)
N	43,610	42,226	43,394

Note: Panel A shows DDD regressions weighted by the county population of children under age six. Panel B shows multilevel models with random intercepts and slopes for counties nested in states. County-year level aggregated data drawn from NCANDS administrative CPS reports from 2003 – 2017. Elig = child eligibility, 1 = age 4, 0 all other ages except age 3, who are omitted from this model. Extday = 1 for whether programs offer an extended day.

Table 2-9: Mandated reporter type

	(1)	(2)	(3)	(4)	(5)	(6)
	Non-Child care/Educational MR			Child care/Educational MR		
	All	Abuse	Neglect	All	Abuse	Neglect
Panel A: Fixed Effects						
SPK	0.03	0.08	0.13	-0.02	0.01	0.04
	(0.02)	(0.09)	(0.08)	(0.03)	(0.07)	(0.05)
CCDF	-0.06	-0.31	-0.20	-0.12	-0.17	-0.20
	(0.11)	(0.32)	(0.20)	(0.11)	(0.16)	(0.19)
HS/EHS	-0.19	0.19	0.02	-0.53	-0.45	-0.67
	(0.15)	(0.64)	(0.48)	(0.33)	(0.29)	(0.37)
N	9,473	9,384	9,427	9,316	8,842	9,063
Panel B: Multilevel model						
SPK	0.04*	0.02	0.124***	-0.01	-0.04*	0.03*
	(0.01)	(0.02)	(0.02)	(0.01)	(0.02)	(0.02)
CCDF	-0.13***	-0.40***	-0.33***	-0.15***	-0.14***	-0.3***
	(0.04)	(0.04)	(0.04)	(0.03)	(0.04)	(0.04)
HS/EHS	-0.17	0.291*	0.39**	-0.54***	-0.08	-0.76***
	(0.13)	(0.15)	(0.14)	(0.11)	(0.14)	(0.14)
N	9,473	9,384	9,427	9,316	8,842	9,063

Note: Panel A shows OLS regressions weighted by the county population of children under age six. Panel B shows multilevel models with random intercepts and slopes for counties nested in states. Enrollment and CPS reports are measured in logs. County-year level aggregated data drawn from NCANDS administrative CPS reports from 2003 – 2017. MR = Mandated reporter.

Appendix

Table 2-A1: Alternative measures of child maltreatment

	(1)	(2)	(3)	(4)
	Full Sample		Preschool Sample	
	Foster Rate	Rate of Subst.	Foster Rate	Rate of Subst.
	All	All	All	All
SPK	-0.02	0.00	-0.02	0.00
	(0.02)	(0.01)	(0.03)	(0.01)
CCDF	-0.02	-0.058*	0.00	-0.053*
	(0.15)	(0.03)	(0.17)	(0.02)
HS/EHS	-0.10	0.03	-0.10	0.02
	(0.20)	(0.05)	(0.22)	(0.05)
N	8,428	9,479	16,581	18,853

Table 2-A2: Effect of ECEC program enrollment on maltreatment, linear time trends included

	(1)	(2)	(3)
	Log(CPS reports)		
	All	Abuse	Neglect
Panel A: Full sample			
SPK	0.02	0.05	0.11
	(0.02)	(0.08)	(0.07)
CCDF	-0.12	-0.24	-0.15
	(0.09)	(0.30)	(0.16)
HS/EHS	-0.12	0.13	-0.31
	(0.14)	(0.39)	(0.23)
N	9,479	9,391	9,432
Panel B: Preschool sample			
SPK	0.03	0.03	0.13
	(0.02)	(0.07)	(0.08)
CCDF	-0.12	-0.15	-0.19
	(0.07)	(0.31)	(0.18)
HS/EHS	-0.35	-0.07	-0.39
	(0.20)	(0.43)	(0.29)
N	18,853	18,218	18,720

Table 2-A3: Test of potential mechanisms, CCDF

	(1)	(2)	(3)	(4)	(5)	(6)
	Income eligibility			Lowest copay		
	All	Abuse	Neglect	All	Abuse	Neglect
Panel A: Fixed effects						
	-0.000*	0.00	0.00	-0.01	-0.011*	0.00
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Panel B: Multilevel model						
	0.00	0.00	0.0001*	0.00	-0.005*	0.00
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
N	6,790	6,747	6,766	6,898	6,856	6,876

Table 2-A4: Effect of ECEC programs on child maltreatment, state-aggregated data

	(1)	(2)	(3)
	All	Abuse	Neglect
Panel A: All children			
SPK	0.12**	0.06	0.21*
	(0.04)	(0.12)	(0.08)
CCDF	-0.02	0.11	-0.03
	(0.17)	(0.14)	(0.13)
HS/EHS	1.32	0.91	0.79
	(0.80)	(0.61)	(0.50)
N	42,339	36,600	39,384
Panel B: Preschoolers only (ages 3 and 4)			
SPK	0.16***	0.074	0.19*
	(0.04)	(0.12)	(0.08)
CCDF	-0.01	0.11	-0.04
	(0.19)	(0.15)	(0.14)
HS/EHS	1.37	0.89	0.63
	(0.85)	(0.64)	(0.47)
N	14,270	12,686	13,212
Panel C: Children under age 3 years only			
SPK	0.10*	0.04	0.22*
	(0.04)	(0.13)	(0.09)
CCDF	0.00	0.13	-0.01
	(0.17)	(0.14)	(0.13)
HS/EHS	1.32	0.82	0.78
	(0.78)	(0.60)	(0.50)
N	27,885	23,595	25,941

Table 2-A5: DDD models, state-aggregated data

	(1)	(2)	(3)
	All	Abuse	Neglect
Panel A: Enrollment Shock			
Jump	0.04	0.05	0.05
	(0.07)	(0.07)	(0.05)
Elig	0.12***	0.32***	0.01
	(0.02)	(0.05)	(0.02)
Jump*Elig	-0.08	-0.12	-0.08
	(0.05)	(0.08)	(0.05)
N	45,731	38,236	42,601

Panel B: UPK Introduction			
Jump	-0.05	-0.01	-0.10
	(0.15)	(0.15)	(0.15)
Elig	0.12***	0.31***	0.01
	(0.02)	(0.07)	(0.02)
Jump*Elig	0.02	0.06	0.02
	(0.04)	(0.10)	(0.03)
N	35,657	30,665	33,161
Panel C: Extended day			
Ext day	0.06	0.13	0.15
	(0.11)	(0.10)	(0.11)
Elig	0.15***	0.32***	0.04
	(0.02)	(0.05)	(0.02)
Extday*Elig	-0.03	0.00	-0.04
	(0.02)	(0.07)	(0.02)
N	32,694	28,106	30,441

Figure 2-A1: Test of pre-trends in child maltreatment reports

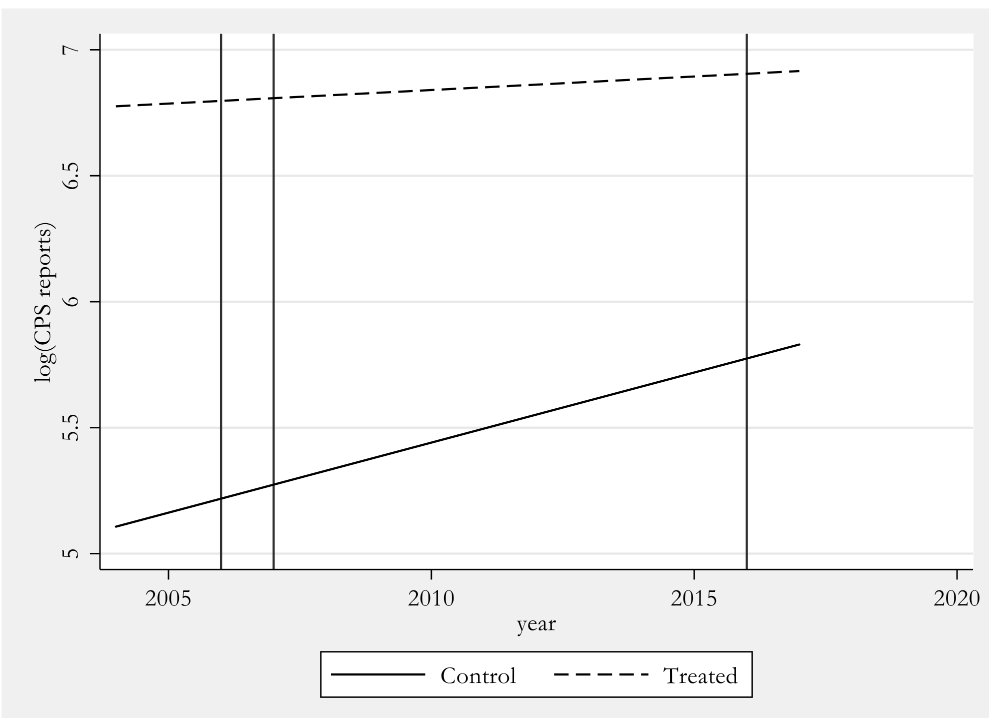
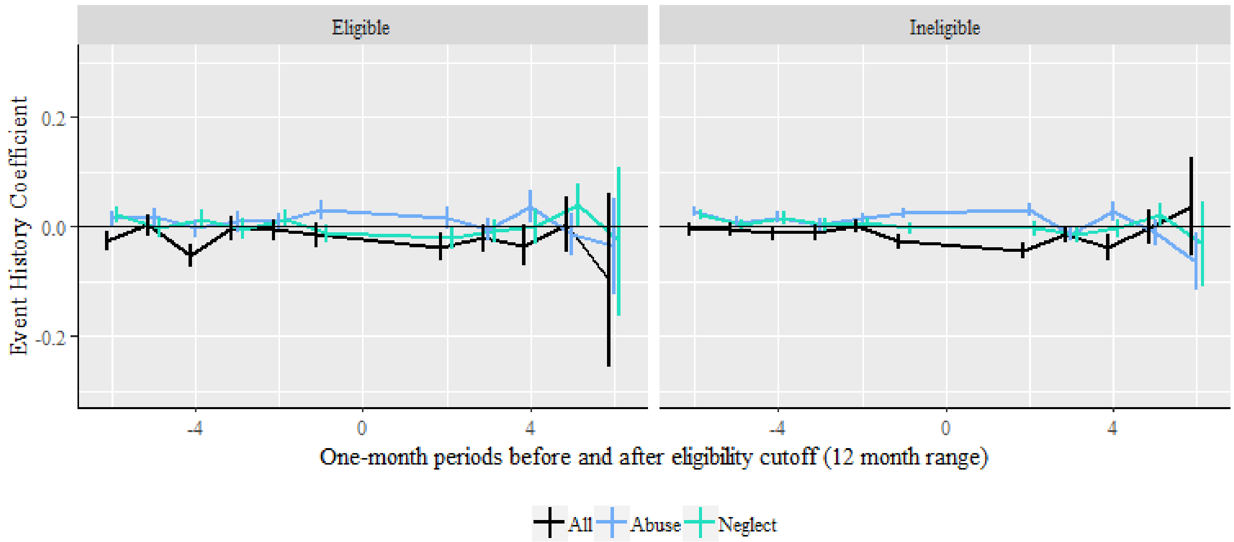


Figure 2-A2: Event History (MLM) effect of SPK/UPK program availability



VIII. Paper 3: Hot Tempered: New Evidence on Temperature and Child Maltreatment

Abstract

A burgeoning literature points to rising temperatures as a cause of widening disparities in cognitive and non-cognitive ability, birth rates, aggression, health, and other correlates of human capital (see e.g. Carleton & Hsiang, 2016). Children in particular are uniquely vulnerable to the insults of climate change. Extreme heat not only affects children directly in terms of infant mortality and early health, parents of young children are increasingly disconnected from protective resources and benefits, cultivating a stressful environment ripe for maltreatment when temperatures rise. Whether temperature directly predicts child maltreatment is unknown. To that end, in this paper I estimate the causal effect of temperature on child abuse by linking daily temperature data to the census of administrative Child Protective Services (CPS) reports for 48 states from 2010 through 2017. In addition to weighted OLS models, I test whether Low Income Home Energy Assistance Program (LIHEAP) state-level cooling support programs and air conditioning (a/c) penetration rates mitigate this effect. Overall, my preliminary results suggest that temperature affects child maltreatment reports, especially among infants younger than 12 months. Though I detect relatively larger effects among reports alleging neglect, that these often capture unsubstantiated abuse does not rule out heat stress as a potential mechanism. Air conditioning penetration rates partially mitigate this relationship, suggestive of large gains from expanding access to cooling support programs such as LIHEAP, though the direct effects of LIHEAP appear minimal at best. This work contributes to nascent knowledge on parenting behaviors and child health, unveiling potential long-term effects of extreme heat among the most disadvantaged populations.

Introduction

Child maltreatment has increased by 12.2 percent since 2013, spurring new research into universal prevention policies and programs (U.S. Department of Health & Human Services Families et al., 2016). Poverty and income have emerged as relatively important structural risk factors for maltreatment based on causal studies linking labor market participation, minimum wages, and other macroeconomic indicators to abuse and neglect (Berger et al., 2016; Cancian et al., 2013; Lindo et al., 2018; Paxson & Waldfogel, 1999a, 2002, 2003b; Raissian & Bullinger, 2017; Wildeman & Fallesen, 2017). The findings from these studies suggest that 'parenting in despair' – that with high levels of stress and depression coupled with limited access to services – is a tractable inequality. By extension, mitigating these effects early in life may plausibly ameliorate the parenting environment and ensuing child safety. That weather and temperature have been linked to crime and violent and aggressive behavior implies that harmful and aggressive parenting may be a natural derivative. Yet, whether these environmental factors known to predict the cognitive and non-cognitive correlates of maltreatment have a direct effect on abuse and neglect is presently unknown.

In this article, I provide new evidence on the effects of temperature on child maltreatment and the effectiveness of air conditioning (a/c) and Low Income Home Energy Assistance Program (LIHEAP) state-cooling policies in mitigating these effects. I first use nonlinear weighted regressions to estimate the effects of temperature on various measures of maltreatment. I then exploit variation in access to LIHEAP and a/c to provide causal estimates of the potential for these policies to offset disparate parenting on account of temperature. My preliminary findings suggest that extreme temperature is indeed linked to child maltreatment reports. I find that a 10 degree increase in temperature is associated with an increase in CPS reports by 4.7 percent, lending credence to the view that heat may affect parenting and/or reporting behaviors. Each additional day of heat exposure over 81 degrees increases CPS reports by .4 percent, or 2 reports per 100,000 children, though a/c penetration halves these effects.

My paper is closely related to the literature on the impact of antipoverty policies on child maltreatment. I extend this literature in three dimensions. First, I demonstrate that extreme heat affects both physical abuse and neglectful parenting, though I find relatively larger effects on neglect. Importantly, an accusation of neglectful parenting does not omit physical abuse, it simply implies that neglect was proven in court or observed by a reporter. Second, I show that expanding LIHEAP – an existing policy – could yield positive externalities in terms of child safety if the policy can spur a/c take-up beyond its current reach, potentially offsetting the notably large expenditures that result from expansion.

My set of findings also extend the literature examining the effect of temperature on early disparities in health. That I detect the first indirect effect of temperature on child maltreatment implies that prior estimates of temperature and human capital projections for children may have been understated. Future research accounting for temperature related disparities should not ignore these indirect effects on children, especially as they may relay information about mechanisms through which other physical health effects operate (infant mortality, injury, etc.).

This paper proceeds as follows. After a short discussion about child maltreatment and the various measures thereof, I describe prior literature in section IV, a/c and LIHEAP in section V, data in section IV, empirical methodology in section VII, followed by my results and conclusion.

Child Maltreatment

Child maltreatment is difficult to measure as the ‘true’ rate is inherently unobservable. Based on the cases that are known and reported to CPS, maltreatment is surprisingly common, with nearly 37.4 percent of children having received a CPS investigation before their 18th birthday (Kim et al., 2017). Young children are particularly vulnerable to maltreatment, making up nearly half of all reports and fatalities (U.S. Department of Health & Human Services Families et al., 2016). Physical and sexual abuse, though among the most widely recognized types of maltreatment, are second and third to neglect, which appears in upwards of 60 percent of CPS reports. Neglect encompasses parental failure to provide food, clothing, shelter, and other factors that prevent harm to a child, as well as the omission of oversight (failure to watch a child to prevent them from harm or injury). Neglect is often used to penalize abusive parenting for which adequate proof is not available, and by extension, is also used to penalize a lack of supervision resulting in physical abuse by another caregiver such as a boyfriend or babysitter.

CPS report rates are widely considered the best measure of maltreatment available, however, these are also thought to under-ascertain the true maltreatment rate as they capture only cases that have been observed and reported. Based on self-reports of maltreatment, the cumulative risk of maltreatment is estimated to be at least three times the rate of CPS confirmed maltreatment (Finkelhor, Turner, Shattuck, & Hamby, 2013; Stoltenborgh, Bakermans-Kranenburg, Alink, & van IJzendoorn, 2015; Wildeman et al., 2014). Further, most cases reported to CPS result in an unsubstantiated finding either through an evaluation or full investigation. Unsubstantiated CPS reports are highly predictive of later substantiated maltreatment (Palusci, Smith, & Paneth, 2005), fatal injuries (Putnam-Hornstein, 2011; Putnam-Hornstein, Cleves, Licht, & Needell, 2013), serious injuries (Schneiderman, Leslie, Hurlburt, Zhang, & Horwitz, 2012), and ED use (Schneiderman, Hurlburt, Leslie, Zhang, & Horwitz, 2012) giving credence to the claim that maltreatment is under-ascertained even among cases known to CPS. CPS agencies are forced to prioritize cases and unless the alleged maltreatment is severe enough, investigators may lack adequate proof to make a substantiated finding. As a result, many high-risk families are turned away, often without prevention services or otherwise.

Prior Literature

As this is the first study to document the relationship between temperature and maltreatment, I document here the ancillary evidence on potential mechanisms through which heat exposure might affect child maltreatment.

Increased temperatures appear to consistently affect physical health and mortality (see e.g. Barreca, Clay, Deschenes, Greenstone, & Shapiro, 2016). As parents with compromised health are more likely to be perpetrators of physical abuse, the burden of illness might compromise non-cognitive stability among caregivers, giving way to more violent and aggressive interactions. A large body of literature links extreme heat and weather with aggressive behavior, criminality, and conflict, the most plausible potential pathway to physical child abuse. Two recent reviews concluded that a 1-SD increase in temperature would increase interpersonal violence by 2.4 to 4 percent (Burke, Hsiang, & Miguel, 2015; Hsiang, Burke, & Miguel, 2013). Similar findings unveiled the role of temperature on aggressive behavior and interpersonal violence (Larrick, Timmerman, Carton, & Abrevaya, 2011). One study found that only extreme temperature shocks predicted violence in the immediate term, followed by a decline in crime in the period after the weather shock (Jacob, Lefgren, & Moretti, 2007). Neuroscientists have long observed depressed cognitive function and other forms of psychological dysfunction as the result of increased temperature as well (see e.g. Seppanen, Fisk, & Lei, 2006). Cognitive and non-cognitive ability and endowments are protective factors for child abuse, through knowledge of child development and the ability to moderate mood or temper. Similarly, a sustained reduction in cognitive and non-cognitive ability might affect violence through an increase in stress, and a cumulative effect on the family budget and the multiple hardships thereof.

Temperature affects children differently than it does adults (Zivin & Shrader, 2016). Children are more vulnerable to temperature in general, though the effects of exposure in utero and during infancy are moderate relative to other shocks. One systematic review unveiled a strong correlation between extreme temperatures and infant mortality, preterm birth, and birthweight (Strand, Barnett, & Tong, 2011). If increased temperature causes a direct increase in infant mortality, then the net effect might be biased by selection, with fewer births of less-healthy children, effectively decreasing child maltreatment on the population level. Alternatively, an increase in preterm birth might reduce mean infant health in the other direction, resulting in more less-healthy children, resulting in an ambiguous overall effect. If temperature causes a reduction in overall infant and child health, we might expect to see an increase in child maltreatment at the mean, as children with poorer health are more likely victims of maltreatment.

Air Conditioning and Cooling-support Programs

Though a/c has steadily grown in prevalence since the 1970s, lagging wages and mounting electricity rates have resulted in vast disparities (Biddle, 2008). Even in the period of this study, a/c penetration in this sample ranges from a mean of 85 percent among renters to 90 percent among families with young children. As young families in particular are uniquely disconnected from resources and have relatively lower wages, their ability to rent or buy newer homes with central a/c or to purchase portable units is relatively constrained. The Low Income Home Energy Assistance Program (LIHEAP) aims to subsidize energy costs to families in the form of reduced energy bills, responding to and preventing energy crisis, and long-term preventative services such as weatherization and home repairs. Authorized under Title XXVI of the Omnibus Budget Reconciliation Act of 1981 (Public Law 97-35), state LIHEAP programs received upwards of $3.69B in appropriations for 2019. Though federal statutes provide some guidance as to benefit eligibility, states are given the flexibility to increase minimum income standards, to offer additional crisis support, and to offer cooling programs (a considerably lower priority in most states than heating support). In general, families with young children under six years are a key target group (along with the elderly and disabled), especially those whose income falls under the federal guidelines of 150 % FPL threshold (or 60 % of state median income).

According to the most recent LIHEAP report to congress (2014), approximately 6.7M households received LIHEAP assistance in 2014, 1M of whom received cooling assistance, with $336 average cooling benefits per household/year, with state payments ranging between $121 (Arkansas) and $1,246 (D.C.). Compared to all households, for whom cooling costs represent 10 percent of their annual expenditures, LIHEAP recipients report spending half as much on cooling. Compared to the 7.5% of all households without cooling, 11.4 % of LIHEAP recipients and 10.9% for all low-income households are without any cooling at all. According to the 2018 LIHEAP state plans, 15 states either offer no cooling support or limit their support such that two or more summer months are missed (11 miss July and August entirely).[14] The remainder offer full cooling programs (26) or some form of summer crisis support. These statistics suggest that

[14] See table 3-A1 in the appendix to see how state LIHEAP offerings vary by month and year between 2014 and 2018. Table 3-A2 shows this same information visually including summer crisis programs as well.

cooling is of a much lower priority, and that the reach for cooling funds is limited. Though LIHEAP cooling programs have substantial reach, low-income households are less likely to receive cooling support in general, and those that do use substantially less cooling support than heating support.

State LIHEAP cooling programs vary in terms of seasonal availability and benefit generosity. For instance, though cooling support is available year-round in Arkansas, similarly warm states, such as Arizona and Alabama offer cooling for the spring/ summer only (April 1 – October 31 and June 1 – September 30, respectively). Benefit levels vary drastically as well. Families in Alabama receive benefits ranging from $305 to $460, while those in Arizona receive anywhere from $75 to $800. As these programs have changed over time in both dimensions, I constructed a database of LIHEAP policies from 2014 – 2018 based on state LIHEAP plans. This database reflects not only the operating dates for state programs in this period as they change over time, it also reflects the turning off and on of funding, allowing programs to go in and out of operation. For instance, Delaware revised their cooling program dates from 2014 to 2015, adding an additional 15 days to the beginning of the period and removing one month at the end in the following year. Georgia introduced a new cooling program in 2018 (that had been abolished in the three years prior), as did West Virginia and Maryland, while Indiana introduced a new program in 2015 and Illinois eliminated their program in 2016. The states without any summer cooling support, including crisis cooling, include Connecticut, Illinois, Kansas, Kentucky, Massachusetts, and Wyoming.

Data

I use several data sets to estimate the effects of temperature on CPS reports. I discuss each in turn.

My core data are drawn from NCANDS administrative child files, which capture the census of Child Protective Services (CPS) reports from 2010 through 2017. Counties with fewer than 1,000 reports are omitted in the restricted sample, limiting generalizability to larger counties with relatively balanced panels. Key outcomes include the log of the overall CPS report rate, each major maltreatment subgroup (abuse and neglect), the rate of substantiated reports, and the rate of removal into foster care. I retain information on key demographic characteristics and report / reporter characteristics as well. Though the raw data capture the exact date of each report, these values are masked in the restricted data such that the first two weeks of the month are coded as the 8th day of the month and the remaining days as the 23rd. For this reason, I aggregate these data to the 8th and 23rd of each month by county x maltreatment type x child age x race such that each observation represents the count of demographic-specific CPS reports in a two-week span. The external data that I describe in the following paragraphs is similarly aggregated.

Temperature data comes from the PRISM Daily Weather Data for Contiguous United States. This series provides the raw daily min and max temperature and total precipitation for the contiguous United States, acquired with permission from the original author.[15] Whereas station-level data is inconsistent due to station closures, reporting gaps, and instrument failures, these data reflect the weighted average temperature and precipitation values for 10 surrounding

[15] Many thanks to Dr. Wolfram Schlenker, Columbia University

stations, reported in 2.5x2.5mi grids that I aggregate to the county using the included geocodes. This allows for a consistent time series that smooths estimates that would be otherwise noisy.

I use SEER for county-level population data (annual) in order to weight my estimates by the county population of children of the same ages. County-level poverty estimates come from SAIPE. Air Quality Index (AQI) data comes from the EPA's Pre-Generated Data Files. These capture the daily AQI as well as the categorical severity. Air conditioning penetration rates come from the American Housing Survey (2010 – 2017), and I scraped the policy variables describing LIHEAP and state cooling programs from the LIHEAP data warehouse.

Empirical Methodology

Temperature and maltreatment

To estimate the effect of temperature on child maltreatment, my primary OLS regression takes the following form:

$$(1)\ Y_{jkt} = \alpha + \gamma TEMP_{jkt} + \delta_1 X_{jkt} + \delta_2 T_t + \delta_3 C_k + \delta_4 AQI_t + \delta_5 AQI2_t + \delta_6 Precip_k + \delta_7 Precip2_k + \theta TREND_{kt} + \varepsilon_{jkt}$$

Where Y is the child maltreatment measure for children living in county k, in state j, in two-week periods, t. Y is one of several measures for child maltreatment. The coefficient of interest, γ, is interpreted as the impact of temperature (TEMP) on child maltreatment outcomes. Temperature is measured in one of two ways. My primary measure is the two-week average max daily temperature for county k in which the child resides. I specify this variable in terms of the raw temperature (F) with a quadratic term and as a series of 10°F bins, representing a count of the number of days in each two-week period with temperatures in each bin. This specification is intended to flexibly allow for nonlinearities in temperature detected in other studies linking health and behavior to climate. In the models with bin measures, I exclude the $70 - 79^{\circ}$F bin to enable an interpretation of relative exposure, or dosage effects.

The vector of covariates, X includes key child demographics (age, race/ethnicity, gender, and maltreatment type). In addition to including year (T) and county (C) fixed effects to account for common correlated effects (such as the recession) and county-specific drivers of maltreatment, respectively, I also include an indicator for season (Season) to account for differences that might drive differences in reporting and temperature. I also test the robustness of my results against the inclusion of county-specific time trends (TREND) to smooth regional trends in maltreatment and to account for within-county compositional changes that might be correlated with temperature (labor market effects not captured by my macro controls, for instance). All regressions are weighted by the county population of children younger than six years to account for relative differences in reporting and observation rates. Following Lee & Lemieux (2010), I use White heteroskedastic-robust standard errors clustered at the state level for computational ease.

The effects of temperature must be identified independent of other markers of climate change that might further aggravate the effects of temperature. As such, I include county-level measures for air quality (AQI) and precipitation (PRECIP) as controls in my primary specifications. As air quality measures are not collected daily by most stations, I use the continuous three-day rolling average Air Quality Index (AQI), testing the robustness of my results against a categorical indicator for air quality levels (good, moderate, unhealthy for sensitive groups, unhealthy, very unhealthy, and hazardous). The AQI is comprised of several pollutant concentrations, though most days rely on particulate matter (< 2.5 microns) and ozone (O3). As precipitation and air pollution have been found in previous literature to have similarly nonlinear effects, I include quadratic terms for each.

My key identifying assumption is that temperature is random and orthogonal to the unobserved determinants of child maltreatment. This is feasible as temperature is unpredictable. Parents or caregivers are unlikely to decide whether to engage in child maltreatment based on prior knowledge of the weather. The density of temperatures, shown in figure 3-2, supports this assertion.

One way to overcome any reservations with this assumption is to employ a Regression Discontinuity (RD) around the start of a heatwave. Though this approach may be feasible in daily data, two-week aggregated data are more limited in that heatwave periods of three or more days cannot be separated from non-heatwave days. A key assumption for RD analysis requires evidence of a discontinuity around the heatwave threshold, on that is untenable in this setting given that most heatwaves do not last as long as one would need for a large enough bandwidth.

Air conditioning penetration

Prior estimates of a/c penetration rates relied heavily on household a/c data from the Decennial Census, which dropped this question from their survey beginning in 1990. I improve upon this method by introducing a novel source of a/c penetration rate data – the American Housing Survey (AHS) covering most years from 2010 – 2017. From 2010 – 2013, I use one variable that asks whether the respondent had any a/c in the home, along with two variables to determine whether the cooling source was central a/c, or portable air conditioners (the number of which are reported). I aggregate the AHS sample to the metropolitan level such that the penetration rate is measured as the fraction of households with any a/c, matched to county-period CPS reports.[16] For 2015 and 2017, I use similar variables that indicate the types of primary and secondary a/c sources in the home. For all years, I generate penetration estimates for all households, those with young children, and for renting households. To estimate whether a/c mitigates the effects of temperature on child maltreatment, I include in my primary specification an interaction term between a/c penetration rates and the measures of temperature.

LIHEAP and maltreatment

As my primary estimates are in the reduced form, I regress a/c penetration rates on LIHEAP program availability at the county level to establish a causal pathway, ensuring that the results I detect in second-stage models are not spurious.

To assess the effect of LIHEAP cooling programs on child maltreatment, I use the same basic model set forth in equation (1) with an interaction between TEMP and an indicator for whether LIHEAP funds were available in the observation period based on the LIHEAP policy variables I describe above. I eliminate the Season indicator and limit the sample to the period between April 1st and September 30th.

To explore whether CPS report rates changed directly as the result of LIHEAP summer cooling programs, I then employ a series of event-history regressions in the periods before and after the start of the LIHEAP cooling period, estimating the following equation:

$$(2)\ Y_{kt} = \alpha + \beta C_k + \rho T_t + \gamma X_{kt} + \sum_{p \in P} \theta_p LIHEAP_{kt}^{p} + \varepsilon_{kt}$$

[16] All counties in a metro area are then assigned the same metro-level a/c penetration rates

Where $LIHEAP_{kt}^{p}$ is a series of dummy variables equal to 1 for the counties in which LIHEAP cooling program was in place for p periods, defined as two-week bins grouped into months, such that P = {-6, -5, -4, -3, -2, -1, 0, 1, 2, 3, 4, 5, 6} months, with the month of the LIHEAP opening as the omitted category (0). I include only states with summer LIHEAP cooling-only programs (crisis-only states and non-LIHEAP states are excluded), though in robustness checks, I group together the non-LIHEAP states to estimate a counterfactual trajectory for the same period. I estimate models with and without county-specific linear time trends, though only report the latter as these represent my more conservative estimates.

Results

The sample of children under age six years follows the expected demographic distributions (table 3-1). Importantly, only 20% of the sample has a LIHEAP cooling policy in place at the time of observation (from 2014 onward) and a/c penetration rates average 87% though have a much larger, right-skewed range. Maximum temperature ranges from 5 – 112, though the average and low temperatures are much lower (not shown). I concentrate on the maximum daily temperature as this aligns with the timing during the day when maltreatment is most likely to occur. The number of CPS reports per child age X race X county X two-week window averages 4.03, though the range stretches from 1 to 159.

Regressing CPS reports on the average maximum daily temperature reveals a statistically significant, nonlinear relationship (table 3-A1 in the appendix). Visual inspection of the raw relationship between these variables is shown in figure 3-3 disaggregated by children's age in years. Interestingly, the relationship appears to be more acute for younger children. Measuring the deviation of the maximum daily temperature from the monthly average follows the same general pattern (figure 3-4), revealing a point of diminishing returns at around 3 degrees. To better understand the role of intensity of exposure, I focus on binned measures of the number of days children were exposed to temperature bins for each two-week period for the remainder of the paper.

Using a binned measure of the number of days in each two-week period children experienced extreme heat (table 3-2), it is clear that relative to the reference category (61 – 70 degrees F), children who experience more days with intense heat are the subject of more CPS reports, regardless of measure, than are children in colder temperatures. As the center of the temperature range (from 31 – 60 degrees F) appears uniform, I collapse these again to five categories I use throughout the paper (table 3-3). Interestingly, the highest heat bin is indistinguishable from the reference category (60-75 degrees F), suggesting that either the effects within this category are masked, or that after 105 degrees, the effect fades. Overall, this estimate suggests that for each additional day in heat above 75 degrees F, CPS reports increase by 3 percent, or 1.5 reports per county X two weeks X child demographics. Table 3-4 shows the same regression disaggregated by children's age in years, revealing the largest effects among the youngest children (columns 1 and 2).

Motivated by these results, I then estimate whether these effects differ based upon children's age. The regression results point to a very consistent relationship, which I then plot in figure 3-5. This figure suggests that relative to the reference category, increased exposure to low-temperature days reduces maltreatment rates for children of all ages. The relationship becomes positive for all age groups at 75 degrees F, however is more intense for younger children. Five year old children see a decline in reports after this point, whereas younger children see a sustained increase of about 2 – 6 reports / 100,000 children, following a decline for all groups when temperatures soar above 105 degrees F. Abuse reports follow the same general pattern, though the effects are much smaller and only positive for children three years and younger (see appendix figure 3-A1). Neglect reports follow the same pattern as all reports (figure 3-A2). Table 3-A3 (appendix) displays this same pattern of result, highlighting the concentration of abuse effects among younger children in the 90 – 105 degree F range.

Overall, these findings suggest that extreme heat is linked to an increase in CPS reports. The relationship is more pronounced and consistent among younger children, who are physiologically vulnerable to heat and to maltreatment and prone to triggering behaviors such as crying, illness, malaise, etc. While it is not possible to parse out these effects in the context of this analysis, future studies might try to observe children and parents under heat-related stress. That heat exposure bears a stronger relationship to neglect reports suggests that 'parenting in despair' is less likely to resolve with physical punishment but not ignorable. For instance, a high-risk family may be more likely to engage in stressful parenting under acute heat (e.g. yelling, shooing children outside, children falling due to lack of supervision), resulting in a neglect report rather than one of abuse. That being said, temperature bears the strongest relationship with abuse among the youngest children, suggesting that both types of maltreatment are subject to heat.

To test whether a/c penetration rates mitigate this effect, I bin a/c penetration rates into quintiles and interact each a/c quintile with each temperature exposure bin. The continuous interaction between a/c penetration and CPS reports (appendix table 3-A2) highlights the effects I display in figure 3-6, but with temperature bins. Among children in high-a/c penetration metro areas (3-5), above the 50th percentile, the relationship between increasing temperatures and CPS reports is uniform. In the lowest a/c penetration quintile, increasing temperatures result in more CPS reports. These findings, together with those in table 3-A2, suggest that a/c penetration rates are indeed effective for those in the lowest a/c penetration rate regions. In figure 3-A3 (appendix) I show the lowest and highest quartiles compared across younger children (younger than 2) and older children (2 – 5 years). These figures follow the same pattern, illustrating the relatively higher CPS report rates in low a/c regions (top panel) compared to that in the highest a/c penetration regions.

LIHEAP is a means-tested program that in some states, offers stipends for recipients to use toward cooling support. In table 3-5, I show the results of a regression where the LIHEAP policy indicator is interacted with each respective temperature exposure bin for three samples, as indicated at the top of the table. The interaction effects are largely null, as is the main effect, suggesting that LIHEAP fails to mitigate the maltreatment-related consequences of extreme heat. Though a regression of a/c penetration rates on LIHEAP availability reveals a small, statistically significant effect (table 3-A4, column 1), this effect loses significance when county-specific linear time trends are included in the model (column 2), suggesting that LIHEAP does not have a first-stage effect on maltreatment. Next, I employ an event history model in order to compare the leads and lags around the time the LIHEAP funding turns on. These results, shown in figure 3-7, imply that the CPS report rates are lower prior to LIHEAP policies turning on, following a peak in the second month followed by a gradual decline. Though the overall report rate falls near to that in the pre-law period, abuse report rates are higher in the post-law period, implying that LIHEAP availability correlates with an increase, rather than a decrease in reports.

Though these findings point to a relationship between heat and CPS reports, it could be that within seasons, any increase in CPS is due to heightened visibility (children playing outside more likely to attract reporter’s attention) rather than maltreatment itself. If the increase in reports I detect is not in fact an artifact of parental behavior but rather that of reporting, any residual effects of temperature on CPS reports should not persist during the summer months alone, when the variation in heat is relatively lower. To test this assertion, I limit my sample to July and August and stratify by whether the report was indeed substantiated, as shown in table 3-6. In panel A, the gradient in reporting among unsubstantiated reports follows the same pattern as it had before, with negative effects relative to the reference category (60-75 degrees) and positive effects in the 90-105 degrees F category. In the substantiated cases (panel B), the coefficients follow a different pattern. In the lowest temperature category, I detect relatively large positive effects on abuse (columns 2 and 5), yet find negative effects on neglect (columns 3 and 6). Coefficients in higher temperature categories are positive for all reports and neglect reports, yet only marginally so for abuse reports, giving further support to the notion that parental behavior is linked to temperature. This could be the case if parents are more likely to fail to provide oversight during high-heat days spurring an uptick in injuries that may spur reporting. As neglect reports may capture only the type of maltreatment for which the burden of proof was adequate, we cannot rule out abuse in these cases as well.

My findings imply that while child maltreatment reports may derive in part from exposure to extreme heat, a/c penetration may be an effective mitigation strategy, especially in low-penetration metro areas. LIHEAP may be a promising solution to increase a/c penetration, though the overall take-up of the program is either too low or dispersed to effect child maltreatment. These results are robust to various specifications and follow the hypothesized patterns in terms of age. The relatively larger effects on neglect, as opposed to abuse, merit investigation in future research.

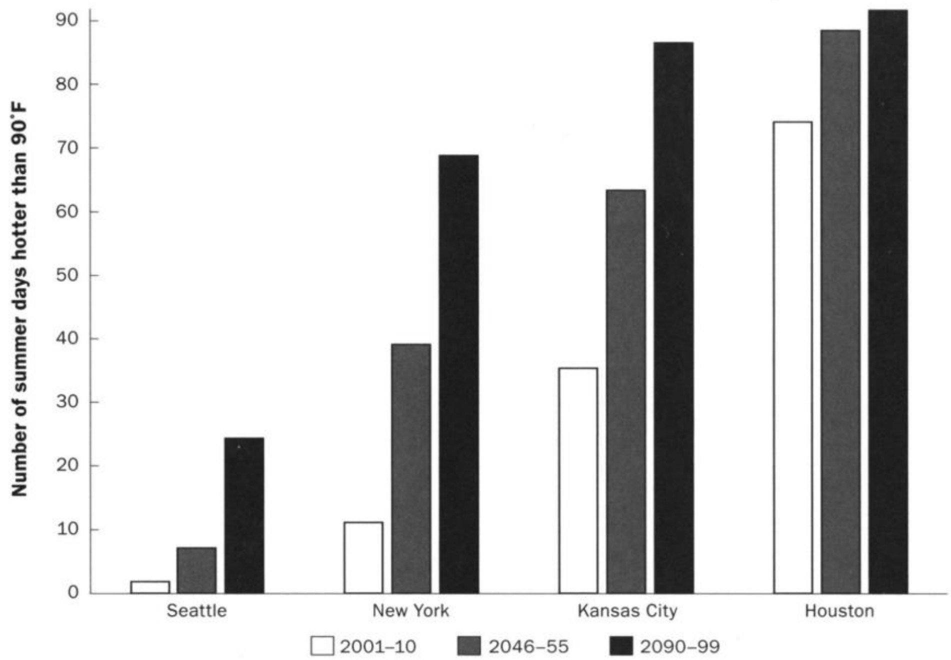
Conclusion

This paper provides the first evidence that temperature and exposure to extreme heat are linked to child maltreatment. My findings imply that a 10 degree increase in temperature increases CPS reports by 4.7 percent. A/c penetration rates nearly halve these effects, implying that expanding access to a/c through cooling support programs may be a promising approach to maltreatment prevention. I detect much larger effects among younger children, for whom the consequences of maltreatment are more salient. As infants account for nearly half of CPS reports alone, mitigating inequalities in a/c access might plausibly improve child safety for this group of children.

Given the projected increases in temperature in the coming years (see figure 3-1), documenting the potential effects is critical to the construction of anticipatory policies, bearing in mind the potential for existing disparities to only further diverge in more extreme conditions. Outsized impacts on young children are of particular concern, given the salience of early life experiences for adulthood health and human capital (Almond et al., 2017; Currie & Rossin-Slater, 2015). Indeed, there is a strong theoretical argument for identifying broad-sweeping policies to improve early child safety that affect health and human capital down the line. That children bear direct and indirect consequences of extreme heat, as I detect here, implies that policymakers should focus funding on the early childhood period. State cooling programs should target their efforts to families with young children, especially those in homes without central a/c. LIHEAP cooling programs are active in four states year-round, in the summer for the majority of states. Six states do not have a cooling program in place at all, and 15 offer support only on a crisis basis (when funding is available). As LIHEAP is an existing policy with in-place funding streams, expanding the reach of support for a/c purchases and installments could reduce maltreatment in the long run. However, the reach of the program is low enough as it stands that I fail to detect an effect.

Figures

Figure 3-1: Projected number of extreme heat days in four US Cities



Note: Each projection is the ensemble average of business-as-usual scenario forecasts for the continental United States.
Sources: (Zivin & Shrader, 2016), adapted from Katherine Hayhoe et al., "Development and Dissemination of a High-Resolution National Climate Change Dataset," Final Report for United States Geological Survey, USGS G10AC00248 (2013); Anne M. K. Stoner et al., "An Asynchronous Regional Regression Model for Statistical Downscaling of Daily Climate Variables," International Journal of Climatology 33 (2013): 2473-94; Melinda S. Dalton and Sonya A. Jones, comps., Southeast Regional Assessment Project for the National Climate Change and Wildlife Science Center ; U.S. Geological Survey (Reston, VA: U.S. Geological Survey, 2010)

Figure 3-2: Identifying variation in temperature

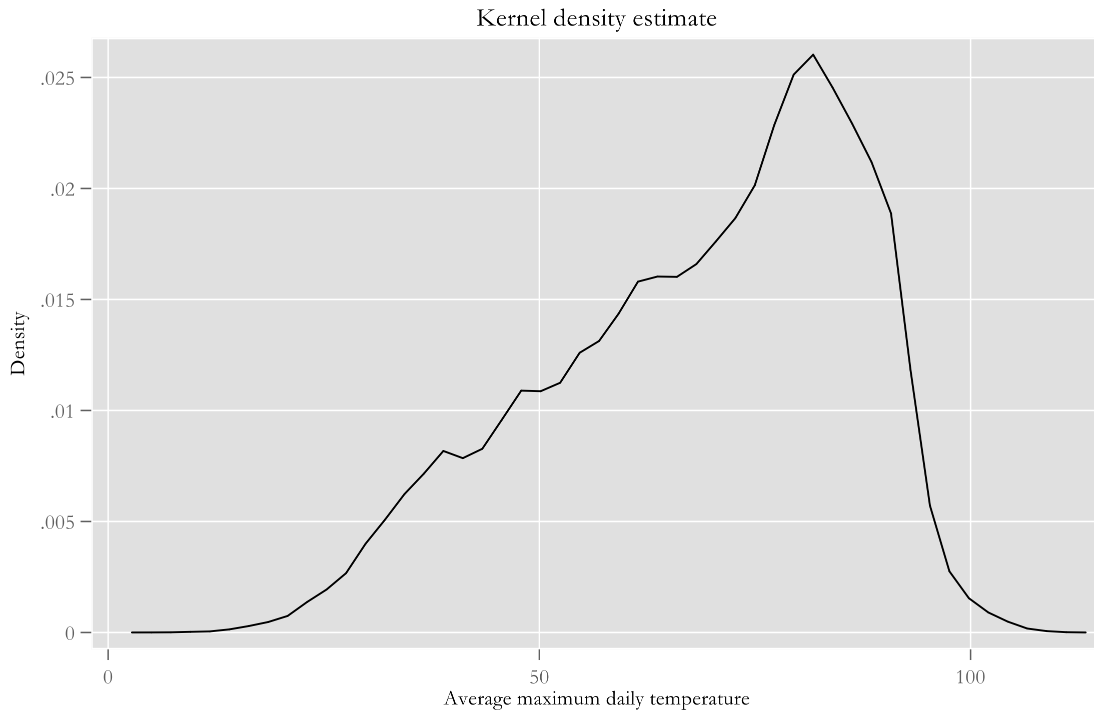
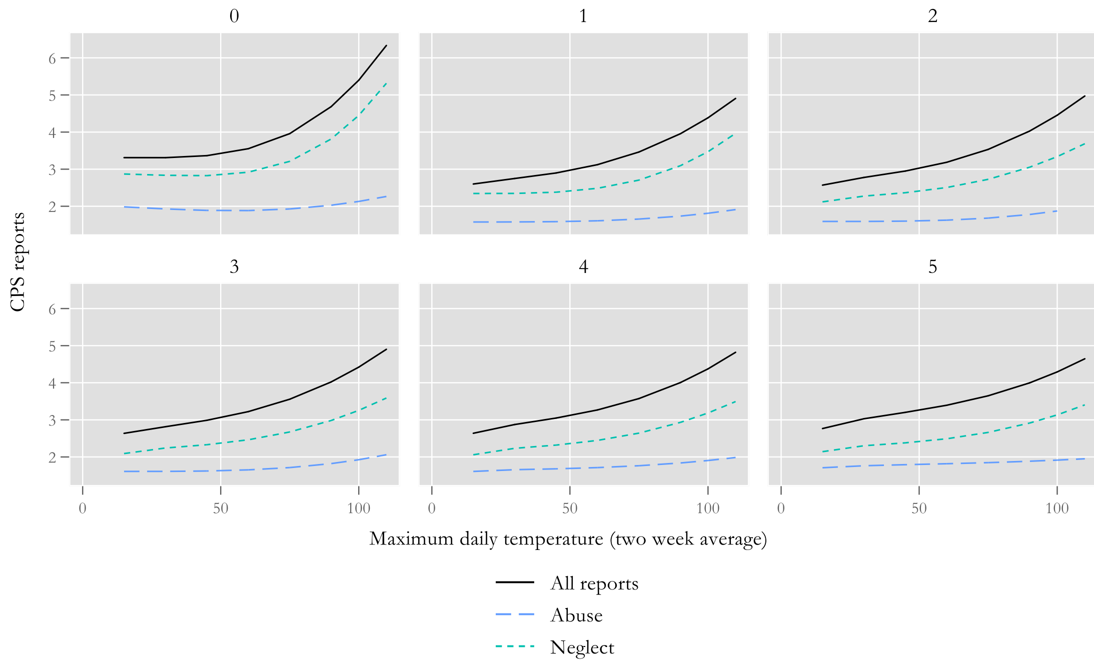
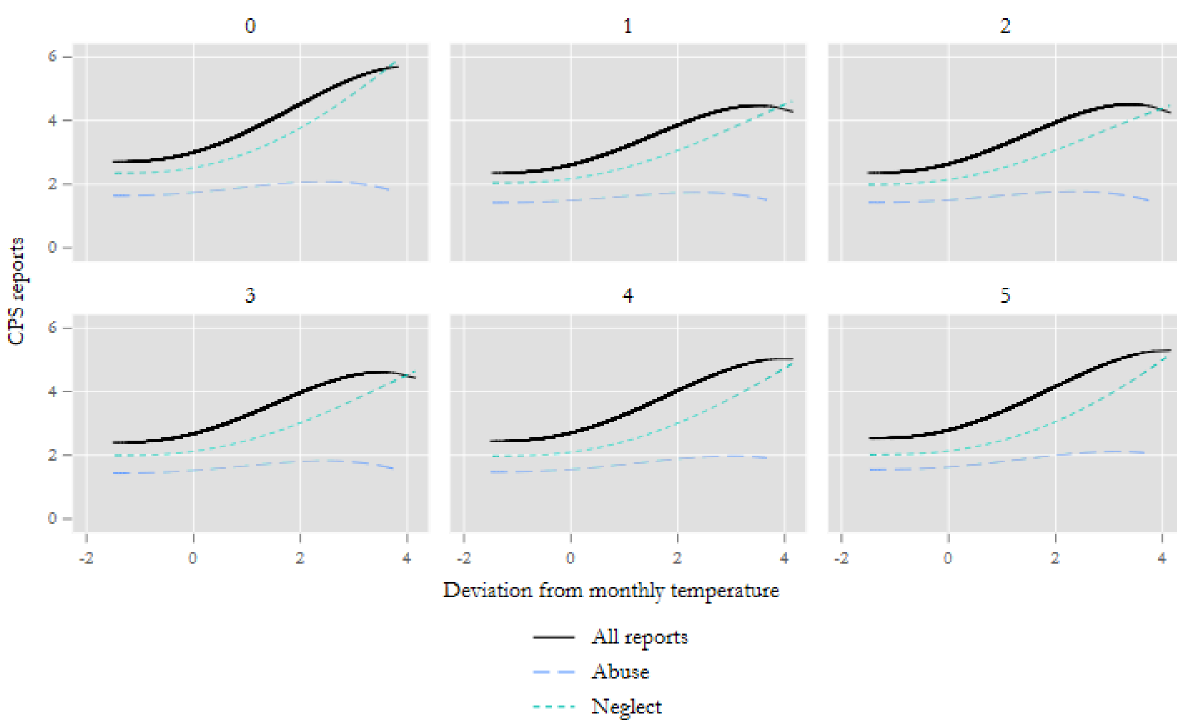


Figure 3-3: Max temperature (bins) and CPS reports (county-two-week total)



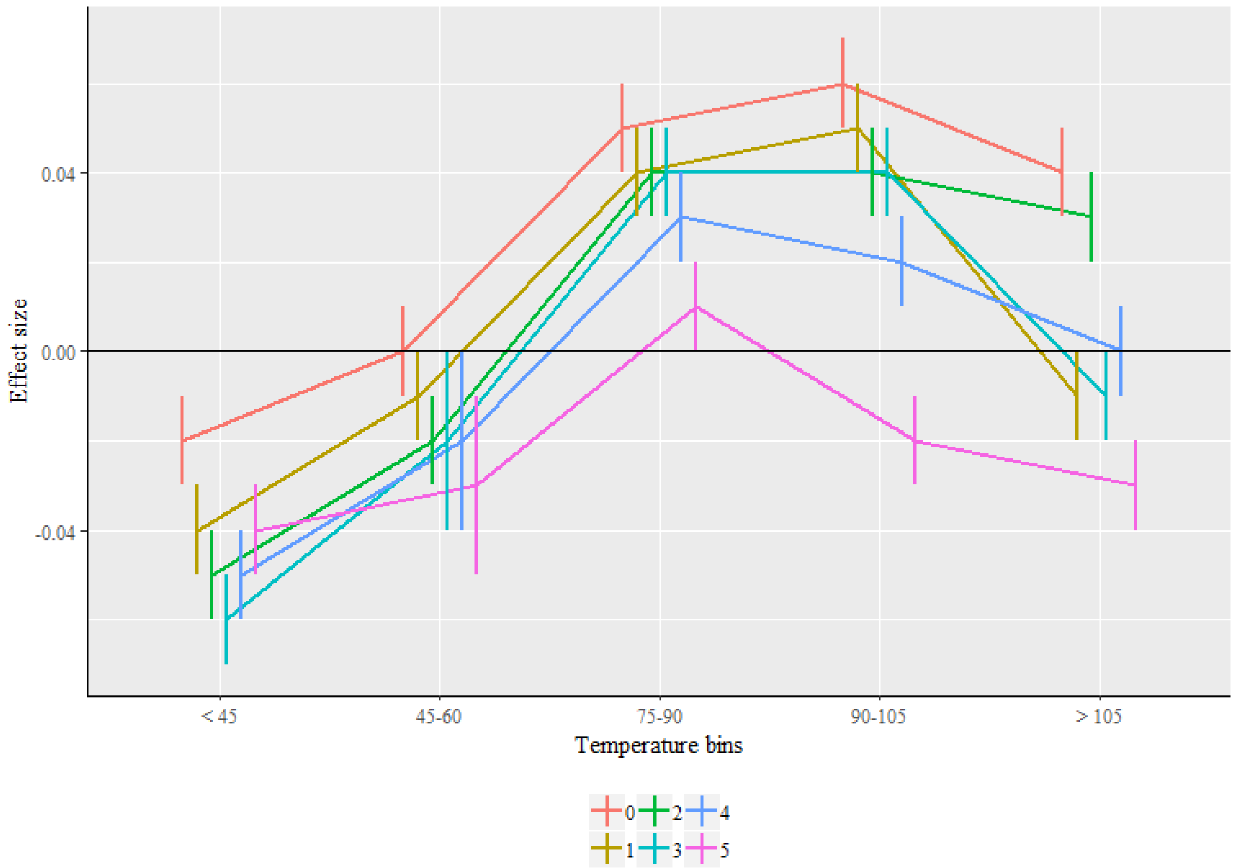
Graphs by child age in years at time of CPS report

Figure 3-4: Deviation from average monthly temperature and CPS reports (county-two-week total)



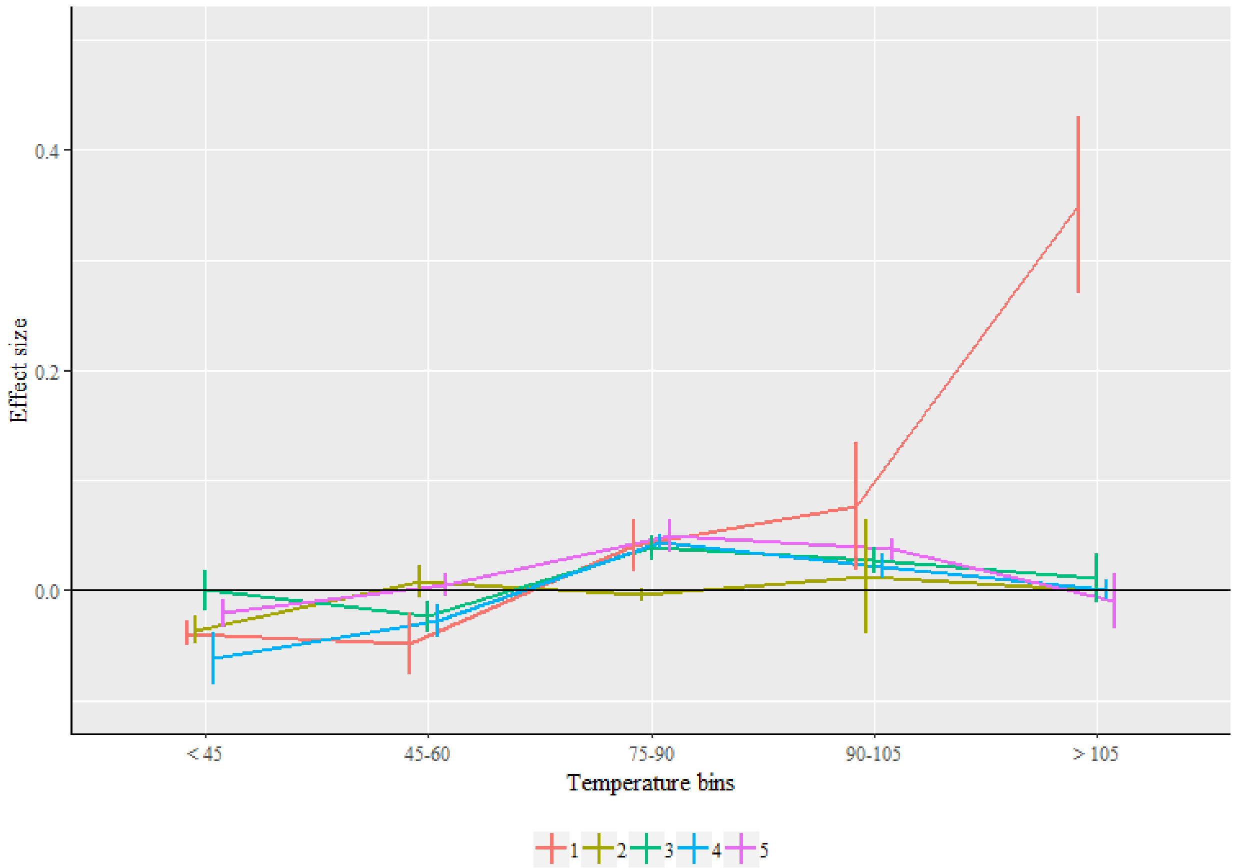
Graphs by child age

Figure 3-5: Temperature and CPS reports / 100,000 children



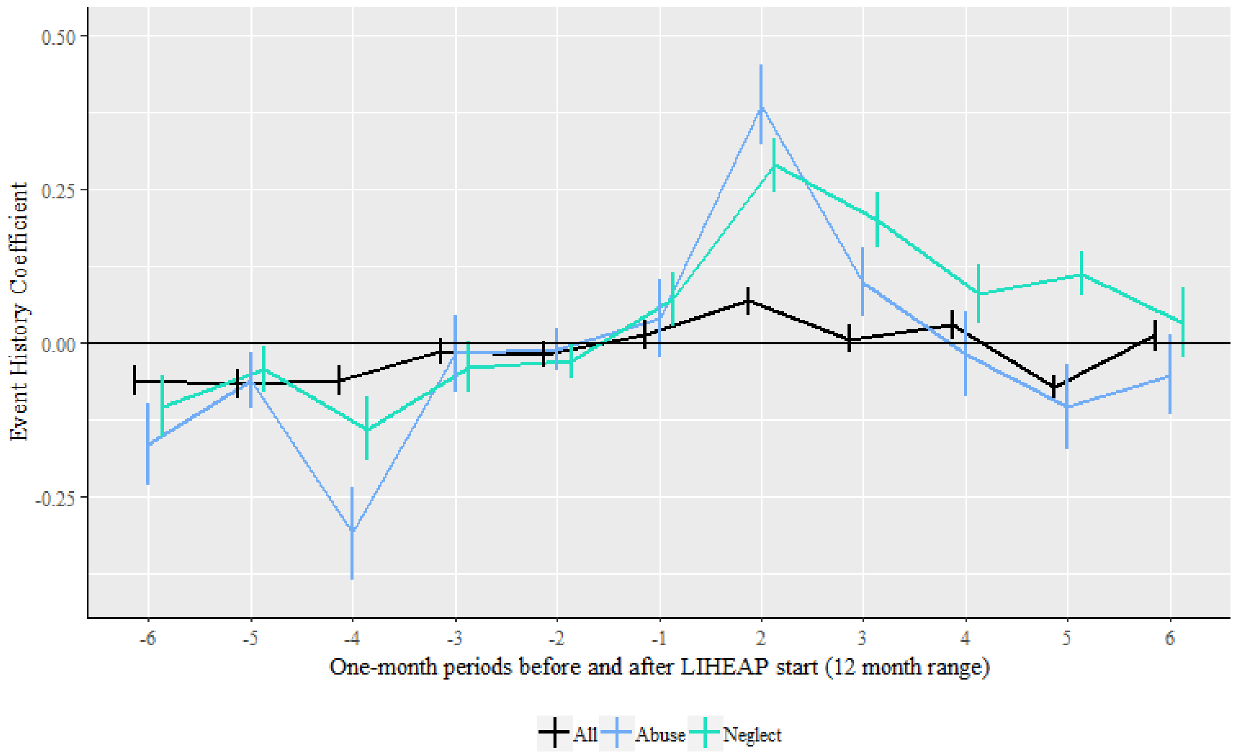
Note: This figure shows the coefficients from separate regressions estimating the effect of days of exposure to temperatures in each of five bins relative to the excluded reference category (60-75 degrees F) by children's age in years. All models include demographic controls, quadratic terms for air quality and precipitation, county, year, and season fixed effects, and county-specific linear time trends. Data are NCANDS administrative CPS reports aggregated to the county-two-week period-age-race-maltreatment level for n=3,098,695 observations in total.

Figure 3-6: AC penetration quintiles and CPS reports/100,000 children



Note: This figure shows the coefficients from separate regressions estimating the effect of days of exposure to temperatures in each of five bins relative to the excluded reference category (60-75 degrees F) by the quintile of a/c penetration rates in a child's metro region (1 = lowest, 5 = highest). All models include demographic controls, quadratic terms for air quality and precipitation, county, year, and season fixed effects, and county-specific linear time trends. Data are NCANDS administrative CPS reports aggregated to the county-two-week period-age-race-maltreatment level for n=3,098,695 observations in total.

Figure 3-7: Event History study of the effect of LIHEAP on CPS report rate / 100,000 children



Note: This figure shows the coefficients from an event history model estimating the effect of LIHEAP availability on CPS reports by maltreatment type. All models include demographic controls, quadratic terms for air quality and precipitation, county, year, and season fixed effects, and county-specific linear time trends. Data are NCANDS administrative CPS reports aggregated to the county-two-week period-age-race-maltreatment level for n=3,098,695 observations in total.

Tables

Table 3-1: Summary statistics

	mean/freq.	min	max
LIHEAP	20.89%	0.00	1.00
a/c penetration	87.20%	0.13	1.00
Max temp	71.98	5.25	112.51
Precipitation (inches)	2.49	0.01	67.57
Air Quality	55.43	0.00	570.00
CPS reports/county/two weeks	4.03	1.00	159.00
Abuse	22.08%		
Neglect	44.28%		

Sexual	8.62%
Psychological	5.60%
Other	5.44%
No Maltreatment	13.99%
Child age (years)	
0	16.81%
1	15.47%
2	16.05%
3	16.78%
4	17.23%
5	17.67%
Child race/ethnicity	
white	30.29%
black	28.60%
Hispanic	27.74%
other	13.37%
Total	3,098,695

Table 3-2: Temperature and CPS reports

	(1)	(2)	(3)	(4)
	Log(CPS reports)	Reports / 100,000 children	Log(CPS reports)	Reports / 100,000 children
Days at max temp (ref = 61 - 70 degrees F)				
< 30 degrees	-0.003***	-0.028*	-0.003***	-0.027**
	(0.00)	(0.01)	(0.00)	(0.01)
31 - 40 degrees	0.00	-0.010*	0.00	-0.011*
	(0.00)	(0.00)	(0.00)	(0.00)
41 - 50 degrees	0.00	-0.01	0.00	-0.01
	(0.00)	(0.00)	(0.00)	(0.00)
51 - 60 degrees	0.00	0.00	0.00	0.00
	(0.00)	(0.01)	(0.00)	(0.01)
71 - 80 degrees	0.003***	0.016***	0.003***	0.017***
	(0.00)	(0.00)	(0.00)	(0.00)
81 - 90 degrees	0.004***	0.020***	0.004***	0.020***
	(0.00)	(0.00)	(0.00)	(0.00)
91 - 100 degrees	0.004***	0.018***	0.004***	0.021***
	(0.00)	(0.00)	(0.00)	(0.00)
101 degrees +	0.00	0.016***	0.00	0.015***
	(0.00)	(0.00)	(0.00)	(0.00)
Child age in years (ref = < 12 months)				

One year	-0.151***	-0.837***	-0.152***	-0.840***
	(0.02)	(0.08)	(0.02)	(0.08)
Two years	-0.143***	-0.820***	-0.143***	-0.822***
	(0.02)	(0.08)	(0.02)	(0.08)
Three years	-0.138***	-0.856***	-0.138***	-0.857***
	(0.01)	(0.09)	(0.01)	(0.09)
Four years	-0.129***	-0.840***	-0.130***	-0.841***
	(0.02)	(0.10)	(0.02)	(0.10)
Five years	-0.107***	-0.738***	-0.107***	-0.738***
	(0.02)	(0.10)	(0.02)	(0.10)
Child race (ref = white/non-Hispanic)				
Black	0.124*	0.03	0.124*	0.03
	(0.06)	(0.18)	(0.06)	(0.18)
Hispanic	0.17	0.06	0.17	0.06
	(0.15)	(0.75)	(0.15)	(0.75)
Other	-0.483***	-1.911***	-0.484***	-1.915***
	(0.09)	(0.40)	(0.09)	(0.41)
Constant	0.083*	8.849***	-0.03	8.093***
	(0.04)	(0.14)	(0.07)	(0.25)
r2	0.36	0.46	0.36	0.46
N	3,098,695	3,098,695	3,098,695	3,098,695
Season FE	X	X	X	X
Year FE	X	X	X	X
County FE	X	X	X	X
County-LTT			X	X

Note: All models include demographic controls, quadratic terms for air quality and precipitation, county, year, and season fixed effects, and county-specific linear time trends. Temperature bins count the number of days in each two-week period a child was exposed to temperatures in the bin range. Data are NCANDS administrative CPS reports aggregated to the county-two-week period-age-race-maltreatment level.

Table 3-3: Temperature (binned) and CPS reports

	(1)	(2)
	Log(CPS reports)	Reports / 100,000 children
Days at max temp (60 - 75 degrees ref)		
< 45 degrees	-0.003***	-0.019***
	(0.00)	(0.00)
45 - 60 degrees	0.00	-0.01
	(0.00)	(0.01)
75 - 90 degrees	0.003***	0.015***

	(0.00)	0.00
90 - 105 degrees	0.003***	0.015***
	(0.00)	(0.00)
>= 105 degrees	0.00	0.00
	(0.00)	(0.00)
Constant	-0.01	8.165***
	(0.07)	(0.25)
r2	0.36	0.46
N	3,098,695	3,098,695
Season FE	X	X
Year FE	X	X
County FE	X	X
County-LTT	X	X

Note: All models include demographic controls, quadratic terms for air quality and precipitation, county, year, and season fixed effects, and county-specific linear time trends. Temperature bins count the number of days in each two-week period a child was exposed to temperatures in the bin range. Data are NCANDS administrative CPS reports aggregated to the county-two-week period-age-race-maltreatment level.

Table 3-4: Temperature and child maltreatment stratified by child age in years

	(1) < 12 months	(2) 1 year	(3) 2 years	(4) 3 years	(5) 4 years	(6) 5 years
Panel A: Log (CPS reports)						
< 45 degrees	-0.00***	-0.00***	-0.00***	-0.00***	-0.00*	-0.00*
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
45 - 60 degrees	0.00	0.00	0.00	0.00	0.00	0.00
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
75 - 90 degrees	0.00**	0.00***	0.00***	0.00***	0.00**	0.00
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
90 - 105 degrees	0.00**	0.00***	0.00***	0.00**	0.00*	0.00
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
>= 105 degrees	0.00	0.00	0.00	-0.01**	0.00	0.00
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Constant	-136.57***	15.82	-112.99***	-53.07***	51.34***	81.29***
	-15.71	-11.37	-11.75	-5.58	-12.89	-12.56
r2	0.43	0.37	0.37	0.36	0.35	0.34
N	542,394	477,281	495,819	515,933	527,417	539,851
Panel B: reports per 100,000 children						
< 45 degrees	-0.01**	-0.02***	-0.03***	-0.03***	-0.02***	-0.01***
	(0.00)	(0.01)	(0.01)	(0.01)	(0.01)	(0.00)
45 - 60 degrees	0.00	-0.01	-0.01*	-0.01	-0.01	-0.01
	(0.00)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
75 - 90 degrees	0.02***	0.02***	0.02***	0.01***	0.01***	0.01

	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
90 - 105 degrees	0.03***	0.02***	0.02***	0.01***	0.01**	0.00
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
>= 105 degrees	0.02***	0.00	0.01	-0.01	0.00	-0.01
	(0.00)	(0.00)	(0.01)	(0.01)	(0.00)	(0.00)
Constant	-1139.26***	246.03***	-1000.27***	-363.28***	238.76***	892.37***
	-44.71	-34.47	-28.94	-31.07	-46.37	-32.76
r2	0.45	0.47	0.47	0.48	0.48	0.47
N	542,394	477,281	495,819	515,933	527,417	539,851
Season FE	X	X	X	X	X	X
Year FE	X	X	X	X	X	X
County FE	X	X	X	X	X	X
County-LTT	X	X	X	X	X	X

Note: All models include demographic controls, quadratic terms for air quality and precipitation, county, year, and season fixed effects, and county-specific linear time trends. Temperature bins count the number of days in each two-week period a child was exposed to temperatures in the bin range. Data are NCANDS administrative CPS reports aggregated to the county-two-week period-age-race-maltreatment level.

Table 3-5: LIHEAP and CPS reports

	(1)	(2)	(3)
	Summer months only	Full year, full sample	LIHEAP states only
< 45 degrees	-0.008**	-0.001**	-0.001**
	(0.00)	0.00	0.00
LIHEAP	-0.049	-0.001	-0.004
	(0.03)	(0.01)	(0.01)
< 45 degrees*LIHEAP	0.059	-0.008**	-0.008**
	(0.04)	(0.00)	(0.00)
45 - 60 degrees	-0.001	0	0.001
	(0.00)	0.00	(0.00)
45 - 60 degrees*LIHEAP	0.001	-0.004***	-0.004**
	(0.00)	(0.00)	(0.00)
75 - 90 degrees	-0.004**	0	0
	(0.00)	(0.00)	(0.00)
75 - 90 degrees*LIHEAP	0.003	0.001	0.001
	(0.00)	(0.00)	(0.00)
90 - 105 degrees	-0.006***	-0.002***	-0.001*
	(0.00)	0.00	(0.00)

90 - 105 degrees*LIHEAP	0.002	0	0
	(0.00)	(0.00)	(0.00)
>= 105 degrees	-0.013***	-0.005***	-0.064***
	(0.00)	0.00	(0.01)
>= 105 degrees*LIHEAP	0.006	0.001	0.060***
	(0.01)	(0.01)	(0.01)
Constant	259.5	-50.968***	-24.904***
	-174.745	-5.697	-6.842
r2	0.25	0.247	0.251
N	1,130,840	2,462,409	1,485,881
Season FE	X	X	X
Year FE	X	X	X
County FE	X	X	X
County-LTT	X	X	X

Note: All models include demographic controls, quadratic terms for air quality and precipitation, county, year, and season fixed effects, and county-specific linear time trends. Temperature bins count the number of days in each two-week period a child was exposed to temperatures in the bin range. Data are NCANDS administrative CPS reports aggregated to the county-two-week period-age-race-maltreatment level.

Table 3-6: CPS reports and temperature, July and August only

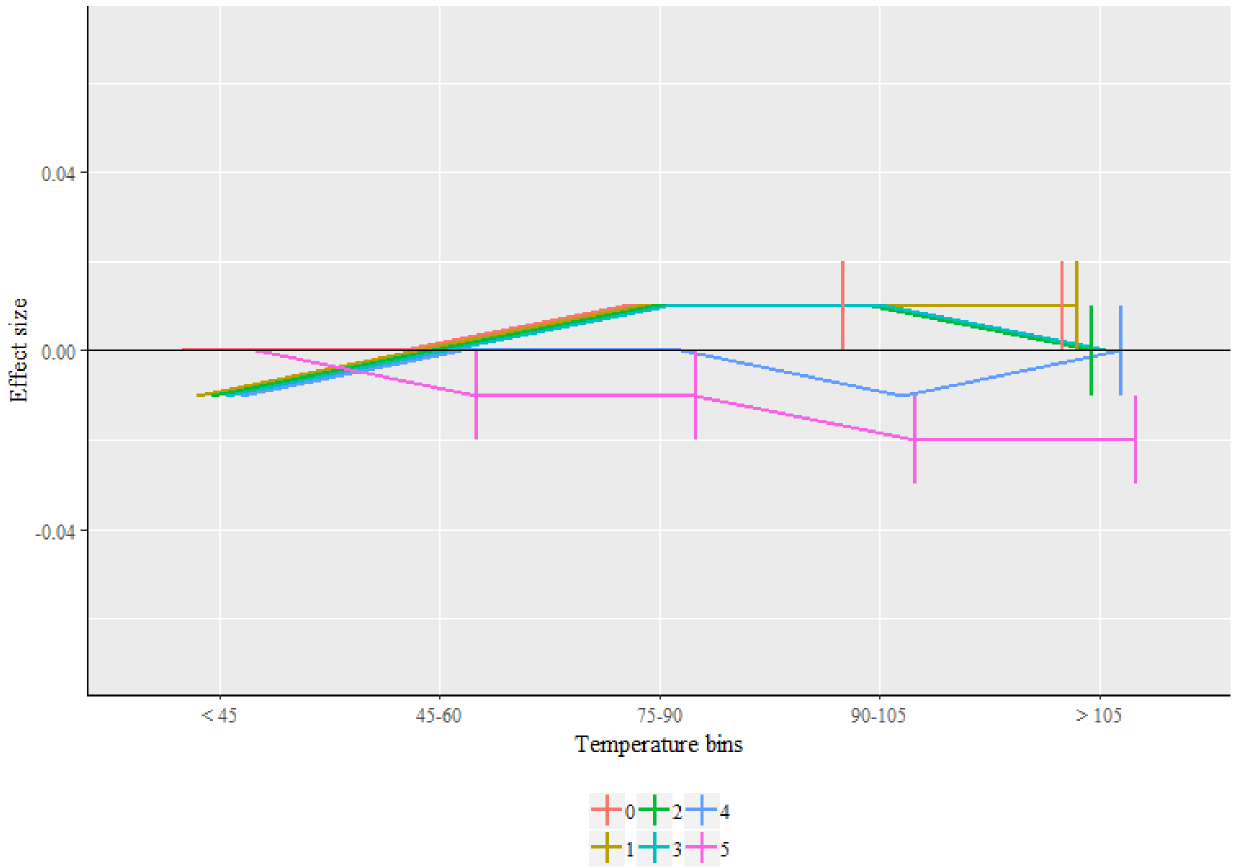
	(1)	(2)	(3)	(4)	(5)	(6)
	Log(CPS reports)			Reports / 100,000 children		
	All reports	Abuse	Neglect	All reports	Abuse	Neglect
Panel A: Unsubstantiated reports						
45 - 60 degrees	0.04***	-0.02***	-0.06***	0.21***	-0.08***	-0.21***
	0.00	0.00	0.00	(0.05)	(0.01)	(0.03)
75 - 90 degrees	0.02***	0.01***	0.02***	0.24***	0.05***	0.15***
	0.00	0.00	0.00	(0.03)	(0.01)	(0.02)
90 - 105 degrees	0.02***	0.02***	0.02***	0.26***	0.06***	0.15***
	0.00	0.00	0.00	(0.04)	(0.01)	(0.02)
>= 105 degrees	0.02**	0.01	0.01	0.23***	0.07**	0.13**
	(0.01)	(0.01)	(0.01)	(0.06)	(0.02)	(0.04)

Constant	82.38*	32.94	91.96***	-139.74	908.19***	401.67*
	(33.21)	(35.71)	(19.59)	(307.97)	(120.34)	(186.83)
r2	0.59	0.43	0.54	0.44	0.59	0.5
N	216678	77673	176819	216678	77673	176819
Panel B: Substantiated reports						
45 - 60 degrees	0	0.06***	-0.01**	0.01	0.16***	-0.03
	0.00	(0.01)	0.00	(0.02)	(0.02)	(0.02)
75 - 90 degrees	0.01***	0.01*	0.01***	0.08***	0.01	0.06***
	0.00	0.00	0.00	(0.01)	(0.01)	(0.01)
90 - 105 degrees	0.02***	0.01*	0.02***	0.08***	0.01	0.06***
	0.00	0.00	0.00	(0.01)	(0.01)	(0.01)
>= 105 degrees	0.01	0.01	0.01	0.06**	0.02	0.04*
	(0.01)	0.00	(0.01)	(0.02)	(0.01)	(0.02)
Constant	-28.53	99.03***	104.33***	371.37*	484.27***	1008.04***
	(23.31)	(16.74)	(29.04)	(138.70)	(29.94)	(145.53)
r2	0.52	0.29	0.48	0.51	0.74	0.56
N	127104	26653	103793	127104	26653	103793
Season FE	x	x	x	x	x	x
Year FE	x	x	x	x	x	x
County FE	x	x	x	x	x	x
County-LTT	x	x	x	x	x	x

Note: All models include demographic controls, quadratic terms for air quality and precipitation, county, year, and season fixed effects, and county-specific linear time trends. Temperature bins count the number of days in each two-week period a child was exposed to temperatures in the bin range. Data are NCANDS administrative CPS reports aggregated to the county-two-week period-age-race-maltreatment level.

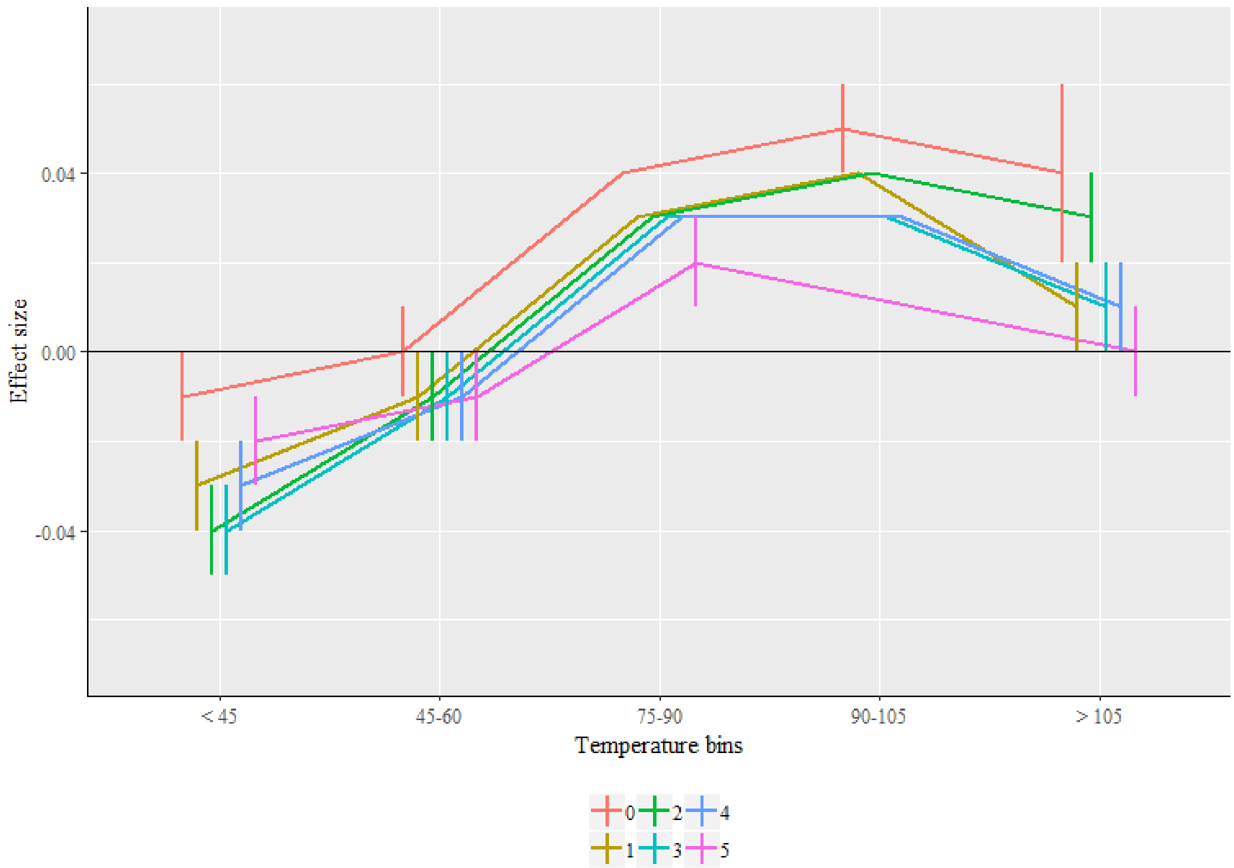
Appendix

Figure 3-A1: Temperature and CPS reports, abuse



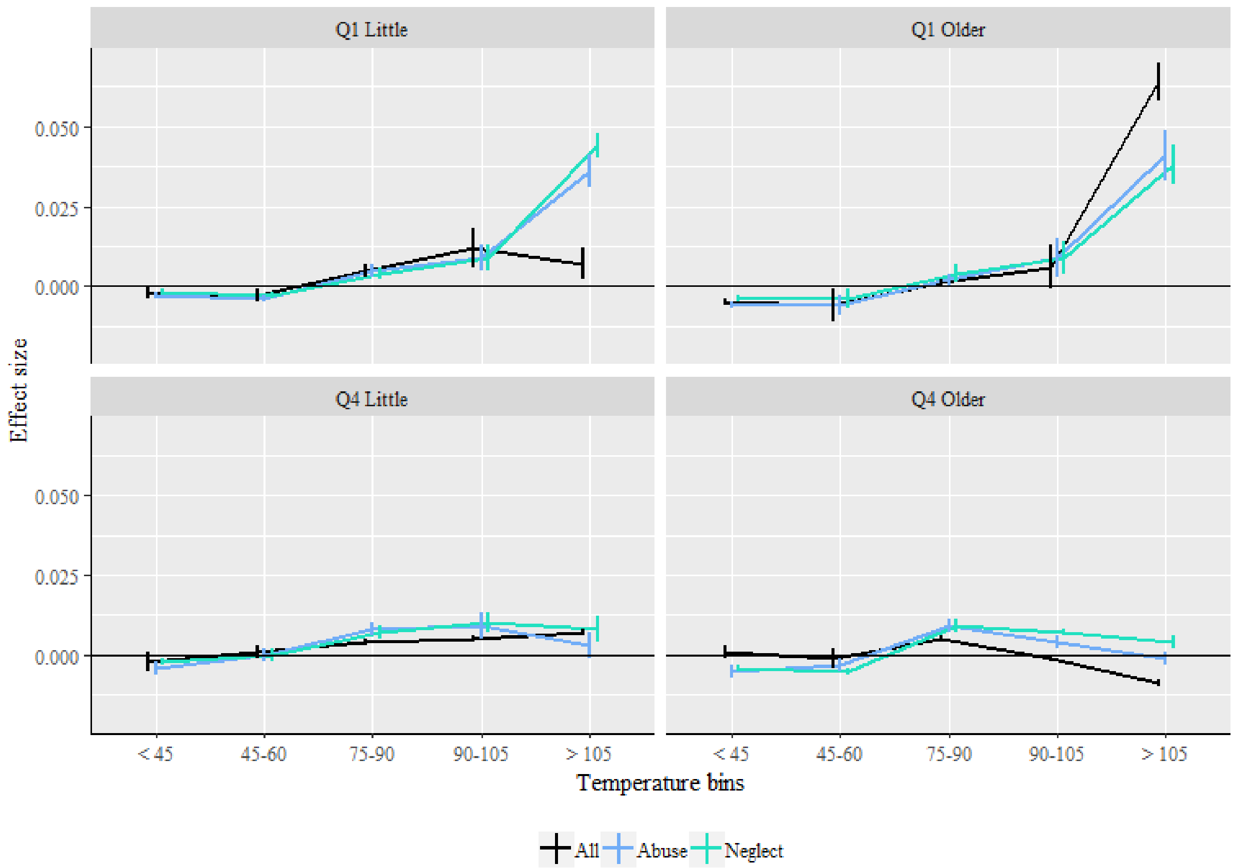
Note: This figure shows the coefficients from separate regressions estimating the effect of days of exposure to temperatures in each of five bins relative to the excluded reference category (60-75 degrees F) by children's age in years. All models include demographic controls, quadratic terms for air quality and precipitation, county, year, and season fixed effects, and county-specific linear time trends. Data are NCANDS administrative CPS reports aggregated to the county-two-week period-age-race-maltreatment level for n=3,098,695 observations in total.

Figure 3-A2: Temperature and CPS reports, neglect



Note: This figure shows the coefficients from separate regressions estimating the effect of days of exposure to temperatures in each of five bins relative to the excluded reference category (60-75 degrees F) by children's age in years. All models include demographic controls, quadratic terms for air quality and precipitation, county, year, and season fixed effects, and county-specific linear time trends. Data are NCANDS administrative CPS reports aggregated to the county-two-week period-age-race-maltreatment level for n=3,098,695 observations in total.

Figure 3-A3: The effect of a/c penetration on CPS reports by temperature bins



Note: This figure shows the relationship between a/c penetration rate quartiles (q1) column 1, and (q4) in column 2, and CPS reports / 100,000 children and days of exposure to each temperature bin. This figure shows the coefficients from separate regressions estimating the effect of days of exposure to temperatures in each of five bins relative to the excluded reference category (60-75 degrees F) relative to a/c penetration rate quartiles. All models include demographic controls, quadratic terms for air quality and precipitation, county, year, and season fixed effects, and county-specific linear time trends. Data are NCANDS administrative CPS reports aggregated to the county-two-week period-age-race-maltreatment level for n=3,098,695 observations in total.

Table 3-A1: Continuous temperature and CPS reports

	(1)	(2)
	Log(CPS reports)	Reports / 100,000 children
Max temperature	0.047***	0.540***
	(0.01)	(0.10)
Max^2	-0.003***	-0.034***
	(0.00)	(0.01)

AQI^3	0.00	0.00
	(0.00)	(0.00)
Precip^3	-0.000***	-0.000**
	(0.00)	(0.00)
Child age in years (ref = < 12 months)		
One year	-0.226***	-1.655***
	(0.02)	(0.13)
Two years	-0.216***	-1.480***
	(0.03)	(0.13)
Three years	-0.214***	-1.407***
	(0.03)	(0.14)
Four years	-0.204***	-1.307***
	(0.03)	(0.15)
Five years	-0.177***	-1.007***
	(0.03)	(0.17)
Child race (ref = white/non-Hispanic)		
Black	0.11	-0.24
	(0.10)	(0.47)
Hispanic	0.12	-0.42
	(0.19)	(2.02)
Other	-0.811***	-4.998***
	(0.11)	(0.98)
Constant	0.427***	25.337***
	(0.04)	(0.52)
r2	0.44	0.35
N	1,959,509	1,959,509
Season FE	X	X
Year FE	X	X
County FE	X	X
County-LTT	X	X

Note: All models include demographic controls, quadratic terms for air quality and precipitation, county, year, and season fixed effects, and county-specific linear time trends. Temperature is measured as the max daily temperature (divided by 10) in each two-week period. Data are NCANDS administrative CPS reports aggregated to the county-two-week period-age-race-maltreatment level.

Table 3-A2: Effect of a/c penetration rates by age, all reports

	(1)	(2)	(3)	(4)	(5)	(6)
	< 12 months	1 year	2 years	3 years	4 years	5 years
Panel A: Log (CPS reports)						
< 45 degrees	-0.01***	-0.01***	-0.01***	-0.01***	-0.01***	-0.01***
	0.00	0.00	0.00	0.00	0.00	0.00
a/c rate	0.08	-0.15	-0.11	-0.65	-0.54*	-0.48

	(0.25)	(0.41)	(0.27)	(0.36)	(0.24)	(0.26)
< 45 degrees*a/c rate	0.01**	0.01*	0.01***	0.01**	0.01*	0.01
	0.00	0.00	0.00	0.00	0.00	0.00
60 - 45 degrees	0	-0.01***	-0.01***	-0.01	-0.01	0
	0.00	0.00	0.00	(0.01)	(0.01)	(0.01)
60 - 45 degrees*a/c rate	0.01	0.01*	0.01	0.01	0.01	0
	0.00	(0.01)	0.00	(0.01)	(0.01)	(0.01)
75 - 90 degrees	0.01	0	0	0	-0.01	-0.01
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
75 - 90 degrees*a/c rate	0	0.01	0.01	0.01	0.01	0.01
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
90 - 105 degrees	0.02**	0.03***	0.02	0.02**	0.01	0.02
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)
90 - 105 degrees*a/c rate	-0.02*	-0.02**	-0.01	-0.01	-0.01	-0.02
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
>= 105 degrees	0.07	0.19	0.25*	0.13	-0.08	0.11
	(0.18)	(0.10)	(0.11)	(0.08)	(0.11)	(0.06)
>= 105 degrees*a/c rate	-0.07	-0.2	-0.25*	-0.13	0.08	-0.11
	(0.18)	(0.10)	(0.11)	(0.08)	(0.11)	(0.07)
Constant	50.25**	30.29*	-10.74	27.75**	21.19*	-20.02*
	(15.23)	(10.63)	(8.79)	(8.03)	(7.90)	(8.11)
r2	0.58	0.48	0.47	0.46	0.45	0.43
N	90,009	80,945	81,582	81,925	82,245	83,576
Panel B: Reports/100,000 children						
< 45 degrees	-0.05***	-0.05***	-0.07***	-0.07**	-0.07***	-0.08***
	(0.01)	(0.01)	(0.01)	(0.02)	(0.01)	(0.01)
a/c rate	-1.27	-2.77	-2.68	-5.30*	-5.44*	-5.49*
	(2.50)	(2.74)	(2.11)	(2.52)	(2.30)	(2.62)
< 45 degrees*a/c rate	0.04***	0.04*	0.05***	0.04	0.05*	0.06**
	(0.01)	(0.01)	(0.01)	(0.02)	(0.02)	(0.02)
60 - 45 degrees	-0.04*	-0.07**	-0.05**	-0.06	-0.07	-0.06
	(0.02)	(0.02)	(0.02)	(0.04)	(0.05)	(0.04)
60 - 45 degrees*a/c rate	0.05	0.06*	0.04	0.05	0.05	0.03
	(0.02)	(0.03)	(0.03)	(0.04)	(0.05)	(0.04)
75 - 90 degrees	0.05	0.06	0.04	0.04	0.02	0.01
	(0.04)	(0.04)	(0.06)	(0.04)	(0.06)	(0.09)
75 - 90 degrees*a/c rate	-0.01	-0.03	0	0	0	0
	(0.04)	(0.04)	(0.06)	(0.05)	(0.07)	(0.10)
90 - 105 degrees	0.23***	0.21***	0.21**	0.15**	0.11	0.09
	(0.04)	(0.04)	(0.06)	(0.05)	(0.07)	(0.10)
90 - 105 degrees*a/c rate	-0.19***	-0.17***	-0.18**	-0.11*	-0.08	-0.09
	(0.04)	(0.04)	(0.06)	(0.05)	(0.07)	(0.09)
>= 105 degrees	0.67	1.25**	1.93**	1.36**	0.64	1.43*

	(0.93)	(0.35)	(0.58)	(0.41)	(0.85)	(0.66)
>= 105 degrees*a/c rate	-0.65	-1.27**	-1.95**	-1.39**	-0.65	-1.47*
	(0.95)	(0.36)	(0.59)	(0.41)	(0.86)	(0.67)
			-	-	-	-
Constant	4.88	-68.05	261.27***	105.15**	158.08***	312.18***
	(70.39)	(35.12)	(31.76)	(27.84)	(28.48)	(34.09)
r2	0.3	0.28	0.27	0.26	0.26	0.25
N	90,009	80,945	81,582	81,925	82,245	83,576
Season FE	X	X	X	X	X	X
Year FE	X	X	X	X	X	X
County FE	X	X	X	X	X	X
County-LTT	X	X	X	X	X	X

Note: All models include demographic controls, quadratic terms for air quality and precipitation, county, year, and season fixed effects, and county-specific linear time trends. Temperature bins count the number of days in each two-week period a child was exposed to temperatures in the bin range. Data are NCANDS administrative CPS reports aggregated to the county-two-week period-age-race-maltreatment level

Table 3-A3: Temperature bins and CPS reports / 100,000 children stratified by child age and maltreatment type

	(1)	(2)	(3)	(4)	(5)	(6)
	< 12 months	1 year	2 years	3 years	4 years	5 years
Panel A: All CPS reports						
< 45 degrees	-0.02**	0.04***	0.05***	0.06***	0.05***	0.04***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
45 - 60 degrees	0.00	-0.01	-0.02	-0.02	-0.02	-0.03
	(0.01)	(0.01)	(0.01)	(0.02)	(0.02)	(0.02)
75 - 90 degrees	0.05***	0.04***	0.04***	0.04***	0.03**	0.01
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
90 - 105 degrees	0.06***	0.05***	0.04***	0.04***	0.02*	-0.02
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
>= 105 degrees	0.04**	-0.01	0.03	-0.01	0.00	-0.03*
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
r2	0.37	0.37	0.37	0.36	0.36	0.34
N	362027	317254	319109	319384	319884	321851
Panel B: Abuse reports						
< 45 degrees	0.00	-0.01*	-0.01**	-0.01*	-0.01	0.00
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
45 - 60 degrees	0.00	0.00	0.00	0.00	0.00	-0.01
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.01)
75 - 90 degrees	0.01*	0.01***	0.01**	0.01**	0.00	-0.01
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.01)
90 - 105 degrees	0.01	0.01***	0.01***	0.01**	-0.01*	-0.02*
	(0.01)	(0.00)	(0.00)	(0.00)	(0.00)	(0.01)
>= 105 degrees	0.01	0.01*	0.00	0.00	0.00	-0.02*
	(0.01)	(0.01)	(0.01)	(0.00)	(0.01)	(0.01)
r2	0.55	0.62	0.62	0.61	0.59	0.55
N	109039	88670	96428	103836	112186	124667
Panel C: Neglect reports						
< 45 degrees	-0.01*	0.03***	0.04***	0.04***	0.03***	0.02***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
45 - 60 degrees	0.00	-0.01	-0.01	-0.01	-0.01	-0.01
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
75 - 90 degrees	0.04***	0.03***	0.03***	0.03***	0.03***	0.02**
	0.00	0.00	0.00	0.00	0.00	(0.01)
90 - 105 degrees	0.05***	0.04***	0.04***	0.03***	0.03***	0.01*

	(0.01)	0.00	0.00	0.00	0.00	0.00
>= 105 degrees	0.04*	0.01	0.03***	0.01	0.01	0.00
	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
r2	0.43	0.46	0.46	0.46	0.46	0.45
N	305600	259763	259227	253525	249115	247748

Note: All models include demographic controls, quadratic terms for air quality and precipitation, county, year, and season fixed effects, and county-specific linear time trends. Temperature bins count the number of days in each two-week period a child was exposed to temperatures in the bin range. Data are NCANDS administrative CPS reports aggregated to the county-two-week period-age-race-maltreatment level.

Table 3-A4: Effect of LIHEAP on a/c penetration

	(1)	(2)
LIHEAP	0.003*	0.001
	(0.00)	(0.00)
Constant	-32.842	-5.367
	(18.56)	(6.29)
r2	0.983	0.818
N	266,904	266,904
Season FE	X	X
Year FE	X	X
County FE	X	X
County-LTT		X

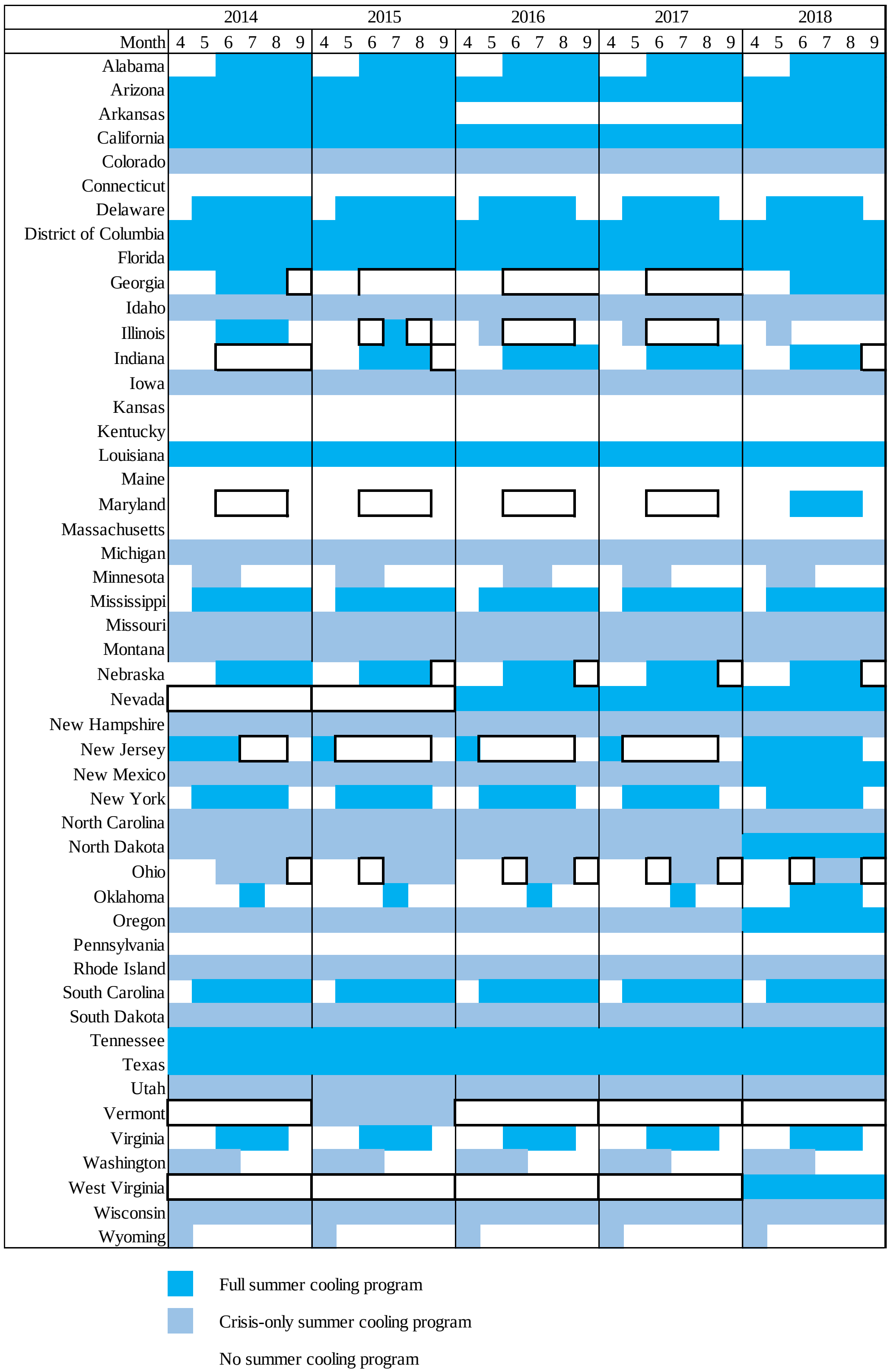
Note: All models include demographic controls, quadratic terms for air quality and precipitation, county, year, and season fixed effects, and county-specific linear time trends. Temperature bins count the number of days in each two-week period a child was exposed to temperatures in the bin range. Data are NCANDS administrative CPS reports aggregated to the county-two-week period-age-race-maltreatment level.

Table 3-A5: State LIHEAP program availability by month, 2014 - 2018

	April		May		June		July		August		September	
	Full	None	Full	None	Full	None	Full	None	Full	None	Full	None
2014	10	22	14	18	19	12	19	14	18	15	14	20
2015	10	21	14	17	17	14	19	13	16	16	13	19
2016	9	23	12	20	16	16	17	15	16	17	12	22
2017	9	23	12	19	16	16	17	16	16	17	12	22
2018	14	21	18	16	25	10	25	11	25	11	17	20

Note: Full = full LIHEAP cooling program, None = no LIHEAP support available (may be available in other months, or as in 6 states, not at all). Statistics represent the 51 contiguous United States. The remaining states offer crisis-only cooling support.

Table 3-A6: State LIHEAP program availability matrix, 2014 – 2018



IX. Conclusion

Child maltreatment is common, with as many as 1 in 8 children experiencing confirmed maltreatment before their 18th birthday (Wildeman et al., 2014). That maltreatment continues to increase in prevalence merits investigation into universal prevention strategies beyond targeted interventions. Policies that improve the parenting environment may be feasible alternatives to those directed at improving parenting behavior. Accordingly, the aim of my dissertation is to examine the externalities of in-place policies on child maltreatment: Medicaid, ECEC programs, and LIHEAP.

In this dissertation, I extend the literature linking poverty to child maltreatment, finding that unequal access to health care and cooling may cause residual stress in the household, and that relieving this disparity could improve child safety. These findings are salient for state policymakers considering an expansion in Medicaid and LIHEAP policies and to those seeking maltreatment prevention strategies. As opposed to targeted, in-home interventions that seek to intervene after a crisis has occurred, the merit of expanding these policies is that they serve to affect a much larger population of at-risk children prior to the point of crisis. In this sense, my estimates are very conservative, as they pertain only to CPS records that measure observable maltreatment. In addition, the benefit of preventing unsafe parenting in early childhood is plausibly much larger in terms of later outcomes in middle childhood and adulthood. Further, that these policies have existing funding is particularly appealing, as scaling up a new intervention or policy – especially one targeted at maltreatment prevention – has proven difficult. The maltreatment externalities of expanding these policies should not be ignored in future cost-benefit calculations.

These findings may serve practitioners as well by providing new evidence on the unseen factors that may spur a CPS report. Caseworkers should refer families to Medicaid as early in a child's life as possible. In the event that the need disappears, the family is equipped to secure insurance regardless of their economic or household circumstances. Though the evidence on ECEC programs is much weaker in general, that young children appear to benefit from HS/EHS and CCDF subsidies implies that early CPS reports with an expressed need for child care assistance should be readily addressed, and seen as a preventative effort, rather than a secondary service. The relationship between temperature and child maltreatment might help CPS agencies anticipate service and staffing needs. Further, ensuring that families have access to LIHEAP or other state cooling support – in addition to all of their other, basic needs – might help reduce later CPS reports for the same families.

Future research should seek to understand the pathways through which this set of policies affects children alongside other work-family and antipoverty policies that improve the household budget and overall stress.

X. References

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